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ABSTRACT

Labor Market Frictions and Spillover Effects from Publicly Announced Sectoral Minimum Wages*

This paper analyzes the horizontal spillover effects of Germany's first sectoral minimum wage. Using a difference-in-differences estimation, I examine the impact of the public announcement and introduction of the minimum wage on sub-minimum wage workers in related jobs outside the minimum wage sector, defined using employment flows. I find an increase in wages and job-to-job transitions for sub-minimum wage workers in related jobs. The spillover effects are driven by workers who reallocate to better-paying establishments, have low labor market experience, and are more closely connected to the minimum wage sector by having former coworkers in that sector.

JEL Classification: J31, J38, J42, J62

Keywords: spillover, outside option, labor market frictions, minimum wages

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1 Introduction

Understanding the effects of minimum wages has been a central topic in labor economics for decades (Dube and Lindner, 2024). Extensive research has examined their impact on directly targeted workers (e.g. Stigler, 1946; Card and Krueger, 1995; Neumark and Wascher, 2008; Dube et al., 2010; Cengiz et al., 2019; Dustmann et al., 2022) and the vertical spillover effects on workers higher up the wage distribution (e.g. Lee, 1999; Gopalan et al., 2021; Fortin et al., 2021; Gregory and Zierahn, 2022; Bossler and Schank, 2023). However, there is limited research on the horizontal spillover effects that can occur when minimum wage policies are not applied universally to all sub-minimum wage workers in the economy. Such horizontal spillovers are particularly relevant when minimum wages are targeted to specific sectors, potentially affecting sub-minimum wage workers in related but non-targeted sectors. As sectoral minimum wages are a widely used policy tool, understanding these horizontal spillovers is crucial. Moreover, examining how workers and firms respond to wage policies when they are not directly targeted provides valuable insights into the broader dynamics of wage setting.

This paper fills this gap by examining the horizontal spillover effects of a sectoral minimum wage introduction on related but non-targeted sectors. My setting is the German main construction sector, which introduced Germany's first sectoral minimum wage following a public discussion and announcement. The institutional context of the main construction sector minimum wage provides a unique case for analyzing horizontal spillover effects for three reasons. First, because the minimum wage was set due to within-sector concerns to reduce wage competition from foreign firms within the European Union in the main construction sector, there were no simultaneous changes in other sectors. Second, I exploit a distinct setting that provides a rare opportunity to isolate and examine wage spillovers across sectors. In this setting, the expected channels for horizontal spillovers — such as the reallocation of workers out of the targeted sector to other sectors due to employment effects — were absent in West Germany (Möller et al., 2011; Frings, 2013; Rattenhuber, 2014; Vom Berge and Frings, 2020).[1] This absence occurred because the minimum wage was intentionally set below the entry-level wages of existing collective bargaining agreements in the main construction sector. This unique setting allows me to focus solely on wage spillovers, assessing whether the minimum wage still affected workers in other sectors despite the absence of these typical spillover mechanisms. Third, the heated negotiations over the minimum wage received considerable media attention, which may have informed workers beyond the targeted sector about this wage policy. Together, these three reasons allow me to measure the causal effects of sectoral minimum wages on similar jobs outside the targeted sector. I analyze the spillover effects using high-quality administrative linked employer-employee data and a difference-in-differences design, focusing primarily on West Germany, where I have more available pre-periods to test for pre-trends than in East Germany.

The difference-in-differences (DiD) design compares the evolution of outcomes for sub-minimum wage workers with those of higher-wage workers outside the main construction sector for a set of jobs similar to this sector over time. Specifically, horizontal spillovers are particularly relevant in industries[2] with sub-minimum wage workers for whom the minimum wage sector represents a potential outside option. I classify these "outside option industries" as industries that had high outflows of low-wage

[1] In Appendix C, I also calculate and replicate the within-sector effects of the minimum wage in the main construction sector on wages and employment. I find no effects on wages and employment at the worker or regional level in West Germany. At the establishment level, I find an increase in average wages, likely due to compositional changes in establishments more exposed to the minimum wage.

[2] "Industries" refer to individual 3-digit entries in the German Classification of Economic Activities, while "sectors" refer to multiple (3-digit or 5-digit) industries collectively covered by a minimum wage regulation.

workers to the minimum wage sector prior to its announcement. I assume that the minimum wage sector and the outside option industries share a common labor market with similar tasks and transferable skills, the only difference being that the minimum wage was enacted only for the main construction sector and is not mandatory for the outside option industries. If the minimum wage had been implemented in the outside option industries, its bite — measured as the percentage of full-time workers directly affected — would have been 14%, compared to the actual bite of 5.8% observed in the main construction sector during the five years preceding the introduction of the minimum wage.

I find horizontal spillover effects of the sectoral minimum wage on sub-minimum wage workers in similar jobs outside the targeted sector. The main construction sector minimum wage led to an on average 3.3% higher wage growth and an increase in job-to-job transitions by 3.1 percentage points for sub-minimum wage workers in outside option industries within the three years after the minimum wage was announced. The results are robust to controlling for region- and industry-specific shocks and are not driven by establishment closure. In an additional robustness check, I utilize a triple differences estimation strategy in which I compare the DiD of outside option industries with the DiD of a set of industries classified as non-outside option industries.[3] Non-outside option industries had no or only very low flows to the main construction sector in the past and are therefore outside the common labor market of the main construction sector and outside option industries. Using non-outside option industries as an additional control group, I am able to effectively control for shocks to the low-wage and high-wage labor market. Using the triple difference strategy, I obtain very comparable results to the baseline DiD specification, highlighting the distinctive influence of the minimum wage in the main construction sector on the outside option industries.

I then address the question of what drives the spillovers. I explore whether these effects stem from individual worker behavior, establishment practices, or particular characteristics of the affected workers, finding that individual switching behavior, especially among more connected and less experienced workers, drives these spillovers. First, I confirm that the spillover effects cannot be attributed to a reallocation of labor from the main construction sector to outside option industries. Second, coworker network effects play a crucial role, with workers more closely connected to the minimum wage sector through former coworkers experiencing higher wage growth and job transitions. Third, I find that the wage spillover effects were driven by workers who changed establishments, not those who stayed in their establishment.[4] In addition, those who did switch establishments did not switch to the main construction sector or to any particular industry, but instead focused their search on better-paying employers and experienced a positive change in wage growth as a result. Fourth, workers with low labor market experience, who likely had less knowledge about their outside options and are potentially more susceptible to changes in outside options and information about those outside options, experienced larger wage spillovers relative to workers with high labor market experience.

In the final step of the analysis, I discuss and provide suggestive evidence on various models of monopsonistic competition in the labor market to rationalize these results. First, models of strategic complementarity are often used to explain spillovers and have strong explanatory power in different contexts (Bhaskar and To, 1999; Bhaskar et al., 2002; Staiger et al., 2010; Berger et al., 2022). Models of strategic complementarity postulate that firms respond to wage changes by other firms to retain workers. The intensity of firms' responses depends on their proximity in terms of non-wage amenities to other

[3]Appendix Figure A.1 illustrates the intuition for this identification strategy.

[4]I also analyze spillovers at the establishment level. Using a similar DiD specification and comparing more exposed to less exposed establishments in outside option industries, I find no change in wages for more exposed establishments, further strengthening the argument that it is the switching behavior of workers, not the behavior of establishments, that drives these results.

firms. Building on this prediction, I provide suggestive evidence that strategic interaction cannot explain the spillovers for West Germany in this context.[5] However, I find evidence for this channel in East Germany, where the minimum wage had a significantly higher bite and, in contrast to West Germany, also had direct effects on the wages of workers in the minimum wage sector. This result suggests that strategic interactions between employers only become relevant when the minimum wage bite is large enough to induce employers to raise the wages of workers within the minimum wage sector itself. Second, to explain the spillover effects in West Germany, I discuss the predictions of the simple general equilibrium model introduced by Jäger et al. (2024). In this model, workers can have information costs that lead to distorted beliefs about their outside options in the labor market, no incentive to search for a job, and a discounted wage while staying in low-wage firms. Two potential sources of information could have informed workers about their outside options during the minimum wage period: workers' networks and extensive media coverage of the intense wage negotiations and the public announcement of the minimum wage. I provide direct evidence for the former source of information, but cannot provide direct evidence for the latter. Supporting an information shock story are the findings of an increase in wage spillovers and job-to-job transitions right in the year of the public discussion and announcement of the minimum wage in 1996 and before its introduction in 1997, a reallocation from lower- to higher-paying establishments, and wage spillovers only for workers with little labor market experience, who presumably have higher information costs and who arguably were less aware of their outside options in the labor market in general and of the already high entry wages in the main construction sector in particular. Finally, because the results were driven by workers who changed establishments rather than staying and receiving a wage increase, the results imply that wage posting rather than wage bargaining is the more dominant mode of wage setting in the low-wage labor market, consistent with findings in e.g. Cahuc et al. (2006); Lachowska et al. (2022).

The central argument and insight of this paper is that horizontal spillovers from publicly discussed and announced sectoral minimum wages on workers in similar jobs outside the minimum wage sector exist. These spillovers materialize by raising wages and reallocating workers from less productive to more productive establishments, thereby increasing the welfare of the economy as a whole. Models of monopsonistic competition provide a theoretical framework for understanding this phenomenon, and I present evidence supporting one such model that incorporates information frictions in the labor market (Jäger et al., 2024). However, this paper does not aim to provide a definitive test of the models of strategic interactions or information friction. Other contexts may be more suitable for evaluating these models and distinguishing between their respective effects.

This paper contributes to an emerging literature on cross-employer spillover effects of wage-setting changes at major employers in three ways (Staiger et al., 2010; Derenoncourt and Weil, 2024; Bassier, 2022).[6] First, by using social security administrative data, I am able to analyze the worker-level spillover response to sectoral minimum wages and reveal wage and reallocation effects that were previously obscured in firm-level studies. Second, this paper proposes a new research design to study individual-level spillover effects for a set of similar jobs using a difference-in-differences strategy. Third, this paper presents a novel institutional context for estimating causal wage spillovers that are not muted by second-

[5] Models of strategic interaction between firms are also rejected in other contexts, such as Roussille and Scuderi (2023); Sharma (2023).

[6] Other related papers include second-order wage spillover effects of decentralized wage bargaining for teachers (Willén, 2021), wage spillovers across establishments within the same firm (Hjort et al., 2020), and market-level effects of privatization of state-owned enterprises (Arnold, 2022). Furthermore, an older literature analyzes the spillover effects of unionization on non-union wages in the same industry due to a "threat effect" or a labor supply shock from workers of the unionized firms reallocating to the non-unionized firms (e.g. Lewis, 1963; Freeman and Medoff, 1981; Neumark and Wachter, 1995; Fortin et al., 2021; Green et al., 2024).

order reallocation effects from the minimum wage sector to other sectors. Moreover, the high-quality administrative data further allows for a comprehensive analysis of the mechanisms underlying the spillover effects.

In addition to the literature on minimum wages and the literature on cross-employer spillovers of wage-setting changes cited above, my paper is related to three other strands of the literature. First, a growing literature studies the role of workers' outside options and their impact on wages and job search (Beaudry et al., 2012; Caldwell and Danieli, 2022; Caldwell and Harmon, 2019; Jäger et al., 2020; Schubert et al., 2023; Jäger et al., 2024; Miano, 2023; Jarosch et al., 2023). Methodologically, I use this literature to define industries for which the minimum wage sectors are potential outside options. Empirically, I add to this literature by presenting causal estimates in a unique setting of wage and information changes in outside options. Second, this paper relates to the literature on the role of labor market institutions in disrupting the coexistence of good and bad jobs (e.g. Acemoglu, 2001; DiNardo et al., 1996; Fortin et al., 2021; Farber et al., 2021). I show that labor market institutions can also have horizontal spillover effects that go beyond the targeted domain. Third, this paper relates to the literature on pay transparency (e.g. Card et al., 2012; Brütt and Yuan, 2022; Baker et al., 2023; Arnold et al., 2023; Cullen, 2023) and information frictions in the labor market (e.g. Skandalis, 2018; Belot et al., 2019; Jäger et al., 2024; Mueller and Spinnewijn, 2023). I add to this literature by showing that wage transparency can potentially be particularly effective in reducing information frictions when it is unsolicited and published prominently in the media.

This paper is organized as follows. Section 2 provides an overview of the institutional setting for sectoral minimum wages in Germany. Section 3 presents the linked employer-employee data and the sampling procedure. Section 4 describes the empirical strategy for estimating spillover effects. Section 5 presents the main results, robustness checks, and mechanisms. Section 6 discusses the findings. Section 7 concludes.

2 Institutional Background

The introduction of the minimum wage in the main construction sector in the context of European trade integration is a unique context to study the horizontal spillovers on sub-minimum wage workers not covered by the minimum wage. This unique context allows for the analysis of spillover effects on worker behavior without simultaneous changes in other sectors or secondary effects of minimum wage policy, such as job losses and reallocation from the minimum wage sector to other sectors. Moreover, the extensive public attention to this minimum wage, communicated through the media, potentially created an information shock for individuals who were unaware of wages for work similar to that in the main construction sector.

Due to European trade integration, sectors in Germany that had been largely spared from international trade up to the beginning of the 1990's were then facing fierce wage competition. European firms could send workers to another EU member state on the terms and conditions of its country of domicile, while domestic firms had to continue to comply with internal regulations (Eichhorst, 2005; Muñoz, 2023). The main construction sector in particular was affected by foreign wage competition. Although there were of course beneficiaries from cheaper construction products in Germany, an opposition to the European posting practice formed relatively quickly with the demand to limit the market opening in order to prevent low-wage competition within the main construction sector.

The main construction sector already had a relatively high collective bargaining coverage in 1995 of approximately 80% establishments covered in West Germany (Möller et al., 2011). To curb wage competition within the main construction sector and set a minimum wage in this sector, collective bargaining agreements could be declared generally binding under Section 5 of the Collective Bargaining Agreement Act[7]. Sectoral minimum wages can be extended to foreign firms through the Posting of Workers Law which came into force in March 1996.

Since there was no minimum wage in the main construction sector to make the Posting of Workers Law effective, representatives of employers and unions in the construction sector debated an appropriate minimum wage rate in 1996.[8] This issue received significant media attention[9] and at times became quite contentious, with various values ranging from 6.14 Euro to 10.35 Euro being proposed and repeatedly publicized in the media as potential minimum wage levels. The debates surrounding this minimum wage centered on establishing a fair wage for this type of work in general while mitigating competitive pressures from foreign construction firms. Negotiations between unions and employers on the level of the minimum wage have broken down several times. The unions even threatened strikes and organized large demonstrations to draw attention to the situation.[10] It is noteworthy for the purposes of this paper that wage levels were a topic of discussion and media attention throughout the year 1996. An agreement on the wage level was eventually reached and the minimum wage was announced on November 16, 1996 in the German Federal Bulletin (No. 215, p. 12102), as required by law, and covered by Germany's most watched news program, the *Tagesschau* (Zubayr and Gerhard, 2005), on November 12, 1996.[11] The two sides (trade union and employer association) agreed on a minimum hourly wage of 8.69 Euro in West Germany and 8.00 Euro in East Germany, which came into force at the beginning of 1997. In mid-1997, the minimum wage in the main construction sector was lowered slightly to 8.18 Euro in West and 7.74 Euro in East Germany and raised again to 9.46 Euro in West and 8.32 Euro in East Germany in mid-1999. Because the main intention of the main construction sector minimum wage was to counter the downward wage spiral induced by foreign firms and labor, the minimum wage had only a minimal impact on the wage structure and employment in the main construction sector (in West Germany) (Möller et al., 2011; Frings, 2013; Rattenhuber, 2014; Vom Berge and Frings, 2020). In Appendix C of this paper, I show and replicate this result by finding no wage and employment effects of the minimum wage within the main construction sector at the regional level or worker level in West Germany. At the establishment level, I find an increase in average wages, most likely due to compositional changes in establishments that were more exposed to the minimum wage.

Taking stock, three features of the sectoral minimum wage in the main construction sector make it particularly valuable for this paper. First, the main construction sector minimum wage was introduced because of within-sector concerns, making it an exogenous variation in outside wages for workers not in

[7]§5 of the Collective Bargaining Agreement Act (*Tarifvertragsgesetz*) states that on request of the collective bargaining parties a collective agreement can be declared generally binding by the federal ministry of labor and social affairs (BMAS). This law requires an agreement of the majority of a bargaining committee of the federal ministry, which consists of three representatives of the employer association and three representatives of the trade union, to pass the general binding declaration. Furthermore, the general binding declaration has to be of public interest and until 2014, the employers bound by the collective agreement must at least employ 50% of the workers in the scope of the collective agreement.

[8]For example, Klaus Schmidt, who was one of three representatives of the employers' association on the bargaining committee of the Federal Ministry of Labor, stated in February 1996 that he would agree to even a minimum wage of 6.14 Euro possibly only with reservations (Glabus, 1996).

[9]For example, the most prominent national daily newspapers in Germany such as the Frankfurter Allgemeine Zeitung, the Süddeutsche Zeitung and other newspapers reported on this topic throughout the year.

[10]As reported by the Frankfurter Allgemeine Zeitung, a newspaper of record in Germany, the relevant trade union for construction (IG Bauen-Agrar-Umwelt) threatened a strike and organized a demonstration with 2,000 officials in North Rhine-Westphalia to push for a minimum wage of at least 10.01 Euro at that time ("Die IG Bau bereitet sich auf Streik vor", *Frankfurter Allgemeine Zeitung*, March 14, 1996, No. 63, p.15). Furthermore, the Süddeutsche Zeitung, another important newspaper of record in Germany, reported of another big demonstration with up to 20,000 construction workers from Bavaria, Baden-Wuerttemberg, and Hesse in Munich citing the trade unions' core demand of 10.01 Euro as a minimum wage ("Der Krieg am Bau weitet sich aus", *Süddeutsche Zeitung*, March 20, 1996, p.33).

[11]https://www.tagesschau.de/multimedia/video/video-229995.html around minute 4:55.

the minimum wage sector. Second, because employment losses and reallocation to other sectors due to the minimum wage were negligible, the minimum wage presents a unique context to study horizontal spillover effects without estimating these second order effects. Third, the extensive public attention to the minimum wage (e.g., through news broadcasts and newspapers) is likely to represent an information shock for individuals who were previously not aware of wages for this type of work.

In the years following the introduction of the minimum wage in the main construction sector, other sectoral minimum wages were also introduced.[12] Table 1 gives an overview of all sectoral minimum wages in Germany, highlighting the importance of this policy tool in Germany over time.[13]

3 Data

To study the horizontal spillover effects of the minimum wage in the main construction sector, this paper uses a high quality linked employer-employee data set. The data allow me to study the worker level effects of horizontal spillovers in a detailed way by following their employment biographies over time. Furthermore, the data allow me to exclude workers and establishments that were directly affected by the minimum wage in the main construction sector, thereby focusing only on the isolated effect on workers in similar jobs. Finally, the data allow me to classify workers and industries that are more or less likely to be exposed to horizontal spillover effects from the main construction minimum wage. Because I have more pre-periods for West Germany to test whether the outcome variables for the treatment and control groups evolved in parallel, I focus on West Germany and present results for East Germany in the appendix.

3.1 The Sample of Integrated Employer-Employee Data (SIEED) 1975–2018

The Sample of Integrated Employer-Employee Data (SIEED) 1975–2018, together with additional establishment level information from the Establishment History Panel (BHP), provides high quality administrative variables. By using the information on establishments, detailed industry codes, wages, and employment biographies of individuals, the data allow me to convincingly estimate spillover effects of sectoral minimum wages in Germany. The SIEED and BHP are provided by the Research Data Centre of the BA at the IAB. Schmidtlein et al. (2020) provide a detailed description of the SIEED.

The main data source of the SIEED is the Employee History (Beschäftigtenhistorik - BeH). The BeH in turn is based on the integrated notification procedure for health, pension and unemployment insurance. This notification procedure started on 1 January 1973 (1 January 1991 in East Germany) and made it mandatory for employers to report information on all of their employees covered by social security to the responsible social security agencies at least once a year. Misreporting is a legal offense. For further details on the notification procedure see Bender et al. (1996). Because the BeH only covers employees subject to social security, civil servants and self-employed individuals or unemployment spells are not included in it.

The SIEED is constructed in a three-step procedure. A 1.5% random sample of the population of establishments in the BeH is taken in the first step. All individuals who worked at least one day in one

[12] See e.g. Popp (2021) for an overview of prerequisites for all sectoral minimum wages in Germany. For the context of this study it is only important that sectoral minimum wages were exogenous from the perspective of workers and firms outside the targeted sectors.

[13] The sectoral minimum wages in industrial laundries (introduced 2009), specialized hard coal mining (introduced 2009), public training services (introduced 2012) and money and value services (introduced 2015) cannot be identified in my data as the 5-digit industry classification that I use in this paper is not granular enough to identify these sectors.

of these establishments between 1975 and 2018 are drawn in the second step. The full employment biographies for these individuals are taken from the BeH in the third step. The employment biographies span the years 1975–2018 and cover employment spells in both sampled and non-sampled establishments. Due to the sampling procedure, the SIEED is representative for establishments in Germany but not for persons. The data contains information on the exact (to the day) spell time period, person and establishment identifiers, personal information such as age, gender, nationality, place of residence, education, the daily wage[14], and type of job (e.g., part-time vs. full-time). To this data, I merge additional establishment level information on the place of work and detailed industry codes from the BHP.

3.2 Sample Construction

Sectoral minimum wages are hourly wages. A drawback of the SIEED is that it does not record an employees' hours worked, which in turn means that exact hourly wages are unknown. To ensure comparability between daily wage rates as an outcome variable and to calculate hourly wages for the definition of treated workers, I proceed in two steps. First, I focus on full-time workers who in general have similar working hours. Second, I set the weekly working hours to 40 hours and then use the daily wages and the imputed weekly working hours to calculate the nominal hourly wages. Using the consumer price index of the Federal Statistical Office, I convert gross daily wages into real wages when using wages as an outcome variable in the analysis.

To identify the national minimum wage sectors, I use the 1973 3-digit, 1993 5-digit, 2003 5-digit and 2008 5-digit German Classification of Economic Activities (WZ). The first four digits in the WZ are based on the Statistical Classification of Economic Activities in the European Community (NACE). Appendix Table A.1 summarizes the industry codes that I use to identify and classify the minimum wage industries. If an establishment has one of the industry codes listed in Appendix Table A.1 during the observation period, I classify it as belonging to the respective minimum wage sector. I use the evaluation studies on sectoral minimum wages in Germany, which were commissioned by the Federal Ministry of Labor and Social Affairs, as aids for delimiting the minimum wage sectors in Appendix Table A.1 (for an overview of these evaluation studies see Fitzenberger and Doerr, 2016). Appendix Table A.2 presents descriptive statistics on the minimum wage sectors. The minimum wage sectors vary widely in terms of their bite (share of workers within a sector with wages below the minimum wage), share of full-time workers, and composition of the workforce, with the main construction sector being the most important minimum wage sector in terms of share in the economy. Since I am interested in spillover effects of sectoral minimum wages and not in the effects on the minimum wage sectors themselves, I omit all observations belonging to a minimum wage sector.

In the data preparation, I largely follow the guide in Dauth and Eppelsheimer (2020). In the empirical analysis, I focus on workers aged 18 to 65. For the main analysis, I focus on West Germany (excluding Berlin) and use the years 1990–98 as the main analysis period (see Section 4).[15] I create an annual panel by selecting all employment spells that include June 30 as the cutoff date, since this date coincides with the measurement of the variables in the BHP. I deal with multiple employment spells of a worker in a year by keeping her main job, defined as the employment spell with the highest wage or longest tenure in case of a tie. I trim extremely low daily wages of full-time workers by dropping observations with real daily wages below the mean real daily wage of the first percentile of real daily wages.

[14]The information on the daily wage is censored by the yearly varying social security contribution, but this does not affect the analysis and results of this paper.

[15]In the Appendix, I also analyze the spillover effects for East Germany and restrict the main analysis period to start from the year 1992 onward.

In the analysis sample, I only keep workers who appeared at least once before and once after the treatment (the public announcement of the sectoral minimum wage).

3.3 Exposed Groups and Descriptives

Workers

I begin by assigning workers outside of the main construction sector to different groups based on the expected intensity of their exposure to the horizontal spillover effects of the minimum wage in the main construction sector. Formally, I assign workers to three wage groups based on their nominal hourly wage in year t. Using the nominal minimum wage in West Germany as a threshold, I define the groups in the following way:

Definition of Wage Groups

	Treated Group	**Partially Treated Group**	**Control Group**
Hourly Wage (in Euro) West	$h_{i,t} < 8.69$	$8.69 \leq h_{i,t} < 8.69 + 40\%$	$8.69 + 40\% \leq h_{i,t} < 8.69 + 80\%$

The variable $h_{i,t}$ refers to the nominal hourly wage of worker i in year t. Although the main construction sector minimum wage was adjusted several times during my observation period, I use only the introductory minimum wage to define the groups because it was mainly this wage that was publicly announced and received greater media attention. I use a partially treated group in this paper mainly for three reasons. First, the adjustments to the minimum wage are covered by the partially treated group, the range of which was defined large enough. Second, because I use imputed hours to calculate hourly wages, the partially treated group could include workers in the treated group who were incorrectly assigned to the partially treated group due to measurement error. Third, the minimum wage in the main construction sector could also affect workers who are just above the minimum wage threshold, for example, because the increased wage in the main construction sector, together with already better non-pecuniary characteristics for some workers, now represents a better deal for these workers.[16] I try different bandwidths to define the partially treated and control group in Section 5.2 and find no qualitative change in the patterns of my results. Using data on the years prior to its introduction (1990–95), Table 2 illustrates descriptive statistics for worker groups affected by the minimum wage outside the main construction sector. These groups differed widely from each other. Workers in the treated groups had a higher share of women, young and low-educated workers and were more likely to work in smaller establishments in rural districts, compared to the control group. In Section 4, I describe how my methodology deals with these issues.

Industries

Furthermore, I also classify industries with workers for whom the main construction sector was considered an outside option (herein: outside option industries) and were therefore more likely exposed to the main construction sector minimum wage. In the empirical analysis, I compare the outcomes of low-wage

[16] This theoretical consideration stems from a model with strategic complementarity that I sketch in Appendix D.

workers to high-wage workers within these industries. To define outside option industries, I use an employment flows approach as in Schubert et al. (2023) and building on Manning and Petrongolo (2017); Nimczik (2023). I begin with constructing the share of separations from a 3-digit industry k to the main construction sector as follows

$$\pi_{k\rightarrow \text{main construction}} = \frac{\#\text{ of separations from industry } k \text{ to the main construction sector in year } t \text{ to } t+1}{\#\text{ of separations from industry } k \text{ in year } t \text{ to } t+1}. \tag{1}$$

I define separations as any employer transition.[17] To construct $\pi_{k\rightarrow \text{main construction}}$, I use only separations of workers who are in the treated or partially treated group in year t and choose the longest possible period from 1985 to 1994.[18] This means that I construct $\pi_{k\rightarrow \text{main construction}}$ for West Germany in a first step and extrapolate it to East Germany (Appendix Figure A.2). Figure 1 illustrates the distribution of $\pi_{k\rightarrow \text{main construction}}$ for the 1992–95 period, weighted by the number of workers in each industry in that time period. This distribution is heavily skewed to the left, with many industries having a low or no share of outflows to the main construction sector. This is as expected, because I use employer transitions instead of industry transitions and the share of the main construction sector in the economy (see Appendix Table A.2) is not too high.

I proceed by classifying industries in the top decile of the employment-weighted distribution (full sample in 1992–95) of $\pi_{k\rightarrow \text{main construction}}$ as outside option industries. Since the outside option industries are the most closely related industries, they share a common labor market with the main construction sector with transferable skills and similar (manual) tasks. Therefore, these industries should be affected by horizontal spillovers from the minimum wage in the main construction sector without being directly targeted by the policy. The Appendix Table A.3 lists the 3-digit industries in the outside option industry classification. As would be intuitively expected, physically labor-intensive industries are classified as outside option industries for the main construction sector (e.g., "manufacture of wooden containers").

4 Empirical Strategy

My main empirical strategy for estimating the horizontal spillover effects of the minimum wage in the main construction sector is a difference-in-differences (DiD) estimator. This DiD estimator compares the evolution over time of different outcome variables for workers earning below the minimum wage with those earning above the minimum wage for the sub-sample of outside option industries.

Because one will typically observe higher wage growth for workers with lower wages compared to workers with higher wages (see Table 2), e.g. due to mean reversion (Ashenfelter and Card, 1982), I focus on the *changes* in outcomes over time rather than shifts in the level of the outcome. I compare the *changes* in outcomes for treated and control group workers within outside option industries over two-year windows (between t and $t+2$), similar to e.g., Dustmann et al. (2022); Currie et al. (1996); Clemens and Wither (2019). In the following, I describe the estimation approach using wages as the dependent variable, but the same arguments apply for other outcome variables as well. Formally, I estimate the following DiD specification for the sub-sample of outside option industries (see Appendix

[17] This accounts for the possibility that for some industries only employers within the same industry are considerable outside options. Defining separations as industry transitions, instead of employer transitions, would thus overstate the role of some industries for workers' job choice.

[18] For consistency, I restrict the West German sample to 1985, since information in the variables was changed from that year onward.

Table A.3) around the time of the public discussion and announcement of the main construction sector minimum wage:

$$
\begin{aligned}
w_{i,t+2} - w_{i,t} = \alpha_i \, + \, \zeta_t \, + & \sum_{t=1990, t\neq 1993}^{1996} \beta_t Treated_{i,t} \times Year_t \\
& + \sum_{t=1990, t\neq 1993}^{1996} \gamma_t Partial_{i,t} \times Year_t \, + \, \delta X_{i,t} \, + \, \epsilon_{i,t}.
\end{aligned} \tag{2}
$$

Here $w_{i,t}$ refers to the log (deflated daily) wage of worker i in year t. In Equation 2, I regress the (deflated daily) log wage growth of worker i between years t and $t+2$ on the interaction of an indicator variable $Treated_{i,t}$, which is equal to 1 if worker i is in the treated group and 0 if worker i is in the control group in baseline year t, and a year indicator $Year_t$. I include a similar interaction term with the indicator variable $Partial_{i,t}$, which equals 1 if worker i falls into the partially treated group at baseline year t and 0 if the worker is in the control group. I include the $Treated_{i,t}$ and $Partial_{i,t}$ indicators in $X_{i,t}$. I also include additional controls in $X_{i,t}$. Specifically, I include 3-digit industry, state, and region type dummies measured at baseline year t. I cluster standard errors at the worker level. To account for time-invariant unobserved worker-specific effects on wage growth, such as ability or motivation to climb up the job ladder, I include worker fixed effects with α_i. ζ_t are year fixed effects. I estimate the DiD specifications including three pre-announcement periods (1990–92, 1991–93, and 1992–94) and three post-announcement periods (1994–96, 1995–97, and 1996–98). The reference period is 1993 to 1995. All estimates are conditional on being employed and earning a positive wage in t and $t+2$. I restrict the sample to individuals with at least one observation in outside option industries during the pre-period base years (1990–93) and one observation during the post-period base years (1994–96) in t. Thus, the sample composition remains fixed, as individuals must work in outside option industries in base year t in one pre-period and one post-period horizon, but are allowed to move out of these industries by year $t+2$ in the post-period.[19] Because I am primarily interested in the effects for the treated group, I will present their results in the main text of this paper while placing the results for the partially treated group in the Appendix. Therefore, in what follows, I discuss the identification assumptions for the treated group, albeit the same arguments likewise hold for the partially treated group.

The coefficient of interest, β_t, compares the *change* in wage growth of the treated versus control group workers within the outside option industries over time relative to the 1993–95 reference period. The change in wage growth for treated versus control group workers within outside option industries for the periods (1990–92, 1991–93, and 1992–94) serves as a placebo test for the parallel growth assumption, indicating that in the absence of a shock affecting the treatment and control groups differently, these groups would have evolved similarly.[20] This would correspond to coefficient estimates β_t that are statistically and economically indistinguishable from zero for the pre-announcement periods.

In Section 5.1, I visualize linear time trends for job-to-job transitions as the outcome variable for low-wage workers compared to high-wage workers. To account for these linear time trends, I follow the state-of-the-art literature on minimum wages in Germany (Dustmann et al., 2022; Bossler and Schank,

[19] I thank an anonymous referee for this suggestion.

[20] Due to a massive migration flow from East Germany to West Germany in 1989 and 1990 (Hunt, 2006; Prantl and Spitz-Oener, 2020), which had substantial negative employment (D'Amuri et al., 2010) and wage effects (Prantl and Spitz-Oener, 2020), I do not estimate the specification for the 1989–91 period because it includes these most affected years. The net inflow to West Germany flattened out in the years after 1990 (Prantl and Spitz-Oener, 2020). I exclude the 1989–91 period to avoid potential bias that could arise from the impact on low-wage workers' wage growth compared to higher-wage workers. With worker fixed effects, the bias from this large labor supply shock could also carry over into the estimation of the coefficients for the other periods.

2023).[21] First, I use estimates of β_t for the pre-announcement years 1990–95 to construct a linear time trend. Second, by comparing the deviations between the estimates of β_t and the updated linear time trend for the post-announcement years, I visually identify any trend breaks in outcomes during the announcement and implementation of the main construction sector minimum wage. I implement this procedure for all outcomes and sub-samples that focus on establishment switchers. In Section 5.2, I discuss the linear time trend assumption in more detail and test its robustness, finding that even large violations of this assumption would still yield valid estimates. In Section 5.3, I introduce a triple difference specification that filters out the linear time trends and finds very similar results to the DiD specification.

By excluding all sectors that introduced minimum wages in t, I eliminate any direct effects of minimum wage introductions, address other minimum wages introduced at the same time, and focus only on the horizontal spillover effects of the main construction sector minimum wage.[22] Furthermore, I assume that other potential simultaneous shocks cannot explain the spillover effects found in this paper. In Section 5.2, I test this assumption by exploiting different types of variation using different sets of fixed effects and by using a triple difference specification to control for shocks to the low-wage or high-wage labor market.

In addition to the event-study analysis in Equation 2, I also estimate the DiD by pooling pre- and post-announcement periods:

$$w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \beta Treated_{i,t} \times Post + \gamma Partial_{i,t} \times Post + \delta X_{i,t} + \epsilon_{i,t}. \qquad (3)$$

The dummy $Post$ equals 0 for the years of 1990–93, and equals 1 for the years 1994–96. To account for group-specific linear time trends in the establishment change outcome variable, I interact the group indicators with a linear time trend $time_t$ when using this outcome. Thus, I assume that any pre-existing trends have been linear and would have continued at the same rate in the absence of the main construction sector minimum wage. The visual event-study specification of Equation 2 provides strong support for this assumption. All other variables remain the same as in Equation 2.

5 Results

Section 5 presents the main results. The publicly announced minimum wage in the main construction sector led to higher wage growth and job-to-job transitions outside that sector for workers in similar jobs. These results were unique effects for sub-minimum wage workers in outside option industries and were not driven by shocks to the low-wage or high-wage labor markets. The wage spillover effects were driven by workers moving to better-paying employers, not by more exposed establishments raising wages for incumbent workers.

[21]Note that the use of linear time trends is also common in the US minimum wage literature (e.g. Dube et al., 2010; Neumark et al., 2014; Meer and West, 2016; Allegretto et al., 2017). More generally, the use of linear time trends is common in the difference-in-difference literature (see e.g. Bhuller et al., 2013; Goodman-Bacon, 2018, 2021).

[22]Note that none of the industries in Appendix Table A.3 introduced a minimum wage during the analysis period. To ensure that I am not estimating any direct effects of the main construction sector's minimum wage, e.g. due to misclassification, I also exclude the (ancillary) construction industries from the outside option industries, and the results of this study remain unchanged (see Section 5.2).

5.1 Wages and Job-to-Job Transitions

In Figure 2, I estimate my baseline DiD specification from Equation 2 using the change in wage growth as the outcome variable.[23] The y-axis shows the DiD coefficients from the interaction in which I compare the change in wage growth for sub-minimum wage workers to workers in the control group for individuals who worked in outside option industries in base year t. I find a sharp increase in wage growth right at the year of the public announcement and discussion of the minimum wage in 1996. Specifically, while the coefficients in the pre-period are not statistically significant, sub-minimum wage workers in outside option industries experienced a statistically significant 2.9% higher daily wage growth over the 1994–96 period relative to the 1993–95 reference period and relative to the control group. The magnitude of the coefficient increased to 3.6% in 1995–97 (the introduction year of the minimum wage) and amounts to 3.1% in 1996–98. In column 1 of Appendix Table A.5, I present the baseline specification illustrated in Figure 2, together with standard errors, the number of observations, and the partially treated group.[24] In Table 3, I estimate the pooled pre- vs. post-period DiD specification of Equation 3. On average, treated workers in outside option industries experienced a 3.3% higher daily wage growth in the post-period relative to the pre-period and relative to the control group.

In frictional labor markets, the publicly announced introduction of the main construction sector minimum wage should lead to an increase in reallocation of workers (e.g. Bhaskar et al., 2002; Jäger et al., 2024). I test this prediction by using the specification of Equation 2 with the change of establishments as the outcome variable. The outcome variable takes the value 0 if the worker did not change establishments from t to $t + 2$ and 1 if the worker did change establishments from t to $t + 2$. Figure 3 illustrates the results.[25] Panel (a) presents the results along with an estimated linear trend based on the coefficients from the 1990–95 pre-period and extrapolated to the post-period. The probability to change establishments for treated workers relative to control workers follows a nearly linear time trend. I assume that this linear time trend of the 1990–95 pre-period would have continued in the absence of the main construction sector minimum wage. Section 5.2 discusses the linear pre-trend assumption in more detail and uses the method of Rambachan and Roth (2023) to examine the possible effects on the post-treatment coefficients of violating this assumption. In Section 5.3, I present a triple difference specification that filters out the linear time trend and shows similar results. Following Dustmann et al. (2022); Bossler and Schank (2023), I detrend the coefficients and confidence intervals in panel (b) of Figure 3 and observe no statistically significant coefficient for the 1990–95 pre-period and a sharp trend break right at the year of the public discussion and announcement of the main construction sector minimum wage in 1996. Specifically, the probability of job-to-job transitions increased by 2.5 percentage points in the 1994–96 announcement period, by 3.7 percentage points in the 1995–97 introduction period and by 2.1 percentage points in 1996–98. Overall, Table 3 illustrates that the probability that more exposed workers decided to leave their job to find a new employer increased by 3.1 percentage points in the post-period relative to the pre-period.

I interpret the sharp increase and positive coefficients of wage growth and establishment change in the post-announcement period for treated workers in outside option industries as horizontal spillover effects of the sectoral minimum wage in main construction. I support this interpretation with further

[23] I present the wage spillover effects in East Germany in the Appendix Figure A.2 and find positive wage spillovers there as well.

[24] I observe a similar spike in wage growth for the partially treated group in 1996. However, the magnitude of the coefficient is much smaller. Since I use the partially treated group mainly to capture measurement errors that may arise from the imputation of hours worked and minimum wage adjustments (see Section 3.3), I focus on the treated group.

[25] Appendix Table A.5 illustrates the results in table form and includes number of observations, standard errors, and the partially treated group.

robustness analyses in Section 5.2 and intuitive results on the mechanisms from Section 5.3.

5.2 Robustness Checks

The difference-in-differences specification of Equation 2 and estimated in Figures 2 and 3 is robust to macroeconomic shocks, mean reversion, and worker-specific unobserved heterogeneity. However, around the time of the introduction of the main construction sector minimum wage, other potential shocks are not captured by my identification strategy and could therefore bias the results. Specifically, one could argue that the increase in wage growth and establishment changes starting in 1996, were driven by group-specific time shocks that were common to all low-wage or high-wage workers at the time. Furthermore, shocks to the control group in outside option industries could be the driver of the results. Other potential shocks that are not captured by the baseline identification strategy could also bias the results, such as city and state specific policy changes, international trade and technological change. I proceed in five steps to probe the robustness of my results to these kinds of shocks. First, I test the robustness of the parallel growth assumption for wage growth and linear time trend assumption for establishment changes and how different changes in the slope of the trend in the post-period alter the estimates. Second, I employ a triple differences estimation strategy. Third, I use an additional control group with higher wages to test for shocks to the base control group in outside option industries. Fourth, I test the robustness of the results to region-, industry-, and occupation-specific shocks. Fifth, I use different definitions of the key independent variables.

Parallel Growth Assumption and Linear Pre-Trends

The negative linear establishment change trends in outside option industries can be attributed to the ongoing decline in employment in manufacturing and routine tasks, as documented in numerous studies (e.g., Autor et al., 2003; Goos and Manning, 2007; Goos et al., 2009, 2014; Dauth et al., 2017). The decline in manufacturing and routine employment continued after 1996, extending at least until 2010 in Germany (Dauth et al., 2017; Bachmann et al., 2019), supporting the assumption that the linear trends continue beyond the analysis period.

Thus, the empirical strategy assumes that the linear trends visualized in Figure 3 for the pre-period would have continued in the post-period with the same slope in the outside option industries in the absence of the main construction minimum wage. In contrast, for the wage growth outcome, the empirical strategy assumes that the potential outcomes of the treatment and control groups would have evolved similarly, with no difference in changes in wage growth, as in the pre-period in the absence of the main construction sector minimum wage. To assess how various violations of the parallel growth and linear time trend assumptions may affect the results, I use the method of Rambachan and Roth (2023).

Rambachan and Roth (2023) propose a novel approach to DiD designs that accounts for potential violations of the parallel growth assumption. Instead of requiring that pre- and post-treatment trends be perfectly identical, their method focuses on constraining the extent to which post-treatment trends can deviate from observed pre-treatment trends. This allows partial identification of the causal parameter of interest even when the parallel growth assumption is not fully met. In addition, Rambachan and Roth (2023) consider the specific case in which violations of parallel growth arise from secular trends that are assumed to evolve smoothly over time. In such scenarios, they propose bounds on the extent to which the slope of the trends may change over time.

Figure 4 illustrates an application of this method to my context. Panel (a) bounds the first post-treatment coefficient for the wage growth outcome by the largest violations of parallel growth in the pre-treatment period. The confidence intervals illustrate the extent of the maximum deviation from parallel trends after treatment, calculated by multiplying the largest pre-treatment deviation by the x-axis value. For example, the confidence intervals at "1" on the x-axis bound the worst case post-treatment difference in trends by the corresponding maximum in the pre-treatment period. Thus, for the first post-period coefficient on wages to be insignificant, the post-treatment trend deviations need to be up to 2.5 times larger than the maximum pre-treatment trend deviation. This amounts to a substantial violation from parallel trends in the post-period and is very unlikely in this (and most other) contexts.

In panel (b) of Figure 4, the x-axis shows various violations of the linear pre-trend assumption. "Original" refers to the coefficient without detrending (as in panel (a) of Figure 3). The value "0" on the x-axis refers to the assumption of exactly linear trends that continue with the same slope as in the pre-period (as in panel (b) of Figure 3). All other values on the x-axis refer to varying increases in the slope of the post-period relative to the pre-period. For example, "0.002" corresponds to a 0.002 increase in the slope of the change in post-period coefficients relative to the pre-period coefficients. For establishment changes, I find that the slope of the post-period counterfactual would have to increase by 0.006 (in amount) to make the first post-period coefficient insignificant. Given a linear trend slope of -0.015 (see Appendix Table A.5), this would correspond to a 40% increase in the slope of the trends for the post-period relative to the pre-period.

To summarize, in order for my main results to become statistically insignificant, the estimated trends would have to be grossly miscalculated or increase sharply in the post-period.

Triple Differences

One concern with the results in Figures 2 and 3 is that the post-announcement effects were not unique to sub-minimum wage workers in outside option industries. Instead, all sub-minimum wage workers (or control workers) may have experienced a shock in the post-announcement period in different types of industries. To control for such group-specific time shocks to the low-wage and high-wage labor markets, I add another control group. Specifically, I define a set of non-outside option industries as industries that are in the lowest decile of the employment-weighted distribution (full sample in 1992–95) of relative 3-digit industry flows to the main construction sector ($\pi_{k\rightarrow\text{main construction}}$). These industries are outside the common labor market of main construction and outside option industries, and thus should not be affected by spillovers from the minimum wage in main construction. However, they serve as a proxy for other shocks to the low-wage and high-wage labor market. Appendix Table A.4 lists the set of 3-digit non-outside option industries.

I proceed by estimating the following triple differences specification:

$$\begin{aligned} w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + &\sum_{t=1990, t\neq 1993}^{1996} \beta_t Treated_{i,t} \times Option_{i,t} \times Year_t \\ + \sum_{t=1990, t\neq 1993}^{1996} &\gamma_t Partial_{i,t} \times Option_{i,t} \times Year_t + \delta X_{i,t} + \epsilon_{i,t}. \end{aligned} \tag{4}$$

The key difference to Equation 2 is that I include a triple interaction of an indicator variable $Treated_{i,t}$, the variable $Option_{i,t}$, and a year indicator $Year_t$. I include all respective double interactions and indicators in $X_{i,t}$. The variable $Option_{i,t}$ is equal to 1 if worker i is employed at an outside option industry (Appendix Table A.3) in year t and 0 if she is employed at a non-outside option industry (Appendix Table A.4). The coefficients of interest, β_t, now essentially compare the difference-in-differences (DiD) of treated versus control group workers in outside option industries relative to the DiD of treated versus control group workers in non-outside option industries.

The triple differences estimates of Equation 4 primarily have two advantages over simple difference-in-differences specifications. First, the triple differences specification confirms the working hypothesis that after the minimum wage was discussed and announced in 1996, workers in industries similar to main construction (outside option industries) should also experience a larger change in their wage growth than workers in industries less similar to main construction (non-outside option industries). Second, the triple differences estimates also remove any group-specific time shocks. Olden and Møen (2022) derive the formal identifying assumptions of the triple differences estimator and show that the estimator does not require two parallel trends assumptions, but only one parallel trends assumption, to have a causal interpretation. Intuitively, any contemporaneous shock to the outcome variable that affects all workers in the treated groups or all workers in the control group across outside option and non-outside option industries will be differenced out. Statistically and/or economically insignificant effects for β_t in the pre-announcement period indicate that the triple differences parallel growth assumption holds. The spillover effect from the main construction sector minimum wage should have only affected workers in the treated group and to a larger extent within outside option industries and therefore does not get filtered out by the triple differences specification.[26] However, it is important to note that the linear time trend also does not get filtered out because low-wage and high-wage workers in non-outside option industries did not follow a similar secular trend as those in outside option industries. This also introduces linear trends with respect to the outcome of wage growth, which must be detrended as well for this specification.[27] This underscores why a DiD comparing low-wage workers in outside option industries to low-wage workers in non-outside option industries is inferior to the baseline DiD of this paper, which compares low-wage to high-wage workers within outside option industries, due to differing trends in non-outside option industries.

Figure 5 shows the results of the triple difference specification of Equation 4. In panel (a), I plot the detrended coefficients and confidence intervals on the change in log daily wage growth. Very similar to the DiD specification in Figure 2, I find no statistically significant pre-trend and a sharp increase in the coefficient right at the year of the public announcement and discussion of the minimum wage in 1996. In panel (b), I show the detrended coefficients and confidence intervals for the change in job-to-job transitions. Again, similar to the DiD specification in Figure 3, job-to-job transitions increased right at the year of public announcement and discussion in 1996.

Taking stock, shocks to the low-wage or high-wage labor market cannot explain the horizontal spillover effects I find in this paper. Rather, the spillover effects were unique to workers in outside option industries.

Control Group Placebo

The spillover effects within outside option industries could be driven by the control group and not the treated group. Although the control group is only meant to represent the counterfactual scenario, and one could also argue that possible shocks to the control group would have happened in exactly the same way to the treatment group in the absence of the minimum wage, it is often worrisome when only the control group drives the effects. To test whether the control group drives the spillover effects, I use another control group with hourly wages higher than the actual control group and estimate the same

[26]For further intuition, Appendix Figure A.1 illustrates the identification strategy.

[27]In Section 5.3, I estimate an alternative triple difference specification that filters out the linear time trends.

specification as Equation 2 but replace the treated group with the control group.[28]

Figure 6 illustrates the results of this placebo exercise. Because there are no statistically significant linear time trends for this wage group for both outcomes, the coefficients are not detrended. I find no statistically significant wage effects or establishment change effects for the baseline control group in outside option industries. Overall, this placebo exercise shows that the spillover effects within outside option industries are not driven by the control group, but rather are distinct effects on sub-minimum wage workers in outside option industries.

Simultaneous Shocks

Alternative hypotheses, such as region-, industry-, or occupation-specific shocks, could also explain the results. To rule out these types of shocks, I exploit different types of variation by using different sets of fixed effects. I present these robustness checks in Table 4. To account for group-specific linear time trends in job-to-job transitions, I interact the group indicators with the linear time trend $time_t$ in Panel B.

I include labor market region (LMR) times year fixed effects in the second column of Table 4. These fixed effects exploit variation within labor market regions across differentially exposed individuals and therefore control for region-specific shocks such as migration shocks to specific labor market regions, city and state specific policy changes, and international trade shocks with different effects across regions. I find that the inclusion of these fixed effects does not change the results. Thus, the positive wage spillover and reallocation effects were not driven by region-specific shocks.

Next, I include 1-digit industry times year fixed effects in the third column of Table 4. These fixed effects exploit variation within 1-digit industries across differentially exposed individuals and therefore control for industry-specific shocks, such as technological change or also international trade shocks and structural changes to the German economy, which affected some industries differently than others. I find that the inclusion of industry times year fixed effects does not change the results qualitatively.

In addition, I include both labor market region by year and 1-digit industry by year fixed effects in the fourth column of Table 4. Again, the positive wage spillover effects and the increase in job-to-job changes are robust to the inclusion of these fixed effects. I also include occupation by year fixed effects in the fifth column and find no change in the results.

In the sixth column of Table 4, I exclude all observations in establishments during their closing year.[29] Demand shocks during the observation period could bias my results. Excluding observations that are affected by establishment closure should capture these shocks on the demand side. I find virtually no change in the coefficients for the wage spillover and reallocation estimations.

Alternative Definitions of Variables

I also test the robustness of my results to different bandwidths of the key independent variables of interest in the last two columns of Table 4. The trade-off between using narrower or wider bands is that narrower bands allow comparisons between treated and control group workers who are more similar to each other, while wider bands make potential identification threats such as spillover to the control group or substitution between groups less likely (Stewart, 2004).

[28]Specifically, I define the partially treated group as workers with hourly wages that amount up to $8.69 + 80\% \leq h_{i,t} < 8.69 + 160\%$ and a second control group with hourly wages that amount up to $8.69 + 160\% \leq h_{i,t} < 8.69 + 200\%$. The variable $Treated_{i,t}$ now takes the value 0 if worker i is in the second control group in year t and 1 if she is in the base control group.

[29]To make sure that these are real establishment closures and not just an establishment takeover or ID change, I use the heuristic in Hethey and Schmieder (2010) and the variables created for it in the BHP.

In the seventh column of the Table 4, I define the partially treated group ($8.69 \leq h_{i,t} < 8.69 + 20\%$) and the control group ($8.69 + 20\% \leq h_{i,t} < 8.69 + 60\%$) with narrower bands. In the last column I define the partially treated group ($8.69 \leq h_{i,t} < 8.69 + 60\%$) and the control group ($8.69 + 60\% \leq h_{i,t} < 8.69 + 120\%$) with wider bands. The coefficients in both cases are very similar to the baseline specification.

A remaining concern may be that the minimum wage sectors have not been delineated well enough. Specifically, the remaining ancillary construction industries in the set of outside option industries (see Appendix Table A.3) could be driving the results and these industries could also be directly targeted by the minimum wage. Although the delineation in Section 3.2 was done conservatively, I still exclude these 3-digit industries (451, 452, 454, and 455) from the set of outside option industries in Figure A.3 and find that the results barely change.

5.3 Mechanisms

Having established that there were indeed unique horizontal spillover effects of the public discussion, announcement, and introduction of the minimum wage in the main construction sector on sub-minimum wage workers in outside option industries, a natural question is what drives the spillover effects. The institutional framework of the minimum wage aimed to mitigate wage competition from foreign firms on German construction sites. Its introduction showed no significant employment impact within the main construction sector in West Germany (see Appendix C, in which I estimate the within-sector effects of the minimum wage in the main construction sector), suggesting limited spillover effects from labor reallocation (Möller et al., 2011; Frings, 2013; Vom Berge and Frings, 2020).

To confirm this, I use a sub-sample of individuals who worked in the main construction sector at time t and use Equation 2 to estimate whether the probability of moving to outside option industries changed from t to $t + 2$. Figure A.5 shows that there has been no change in flows from the main construction to outside option industries over time. Thus, a change in reallocation of workers from the main construction sector to outside option industries cannot explain the spillover effects.

Network Effects

One driver of the estimated spillover effects could be workers who are more closely connected to the minimum wage sector through having former coworkers in that sector. For example, former coworkers in the main construction sector could be a source of information on any news in that sector for workers in outside option industries (Caldwell and Harmon, 2019).[30] In line with this, I expect that individuals with a former coworker in the main construction sector were, on average, more informed about the new main construction sector minimum wage than an individual without such an additional source of information. Furthermore, the triple difference estimation that this test employs has the advantage of filtering out any group-specific time shocks *and* linear trends within the outside option industries, thus providing an additional robustness check.

I define the worker-coworker network as follows. For each worker in the analysis sample, I know the establishment and the exact start and end dates of a job at the establishment. I then create a list of coworkers who worked in the same establishment, in the same 3-digit occupation, and at the same time as the focal worker. These employees form a worker-coworker network. Since information on the total workforce is only available for panel establishments in the SIEED, I only use these establishments

[30] See also Granovetter (1974). Recent surveys on network effects include Ioannides and Loury (2004) and Topa (2011).

to define the network. In the final step of data preparation, I use the sub-sample of main construction workers[31] to define a binary network variable. This variable takes the value 1 if the focal worker had a former coworker who worked in the main construction sector during the analysis period 1990–98, and zero otherwise.

To analyze whether workers in my analysis sample with former coworkers in the main construction sector have experienced higher spillover effects, I estimate the following equation (similar to Equation 4):

$$
\begin{aligned}
w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + &\sum_{t=1990, t\neq 1993}^{1996} \beta_t Treated_{i,t} \times Network_i \times Year_t \\
+ &\sum_{t=1990, t\neq 1993}^{1996} \gamma_t Partial_{i,t} \times Network_i \times Year_t + \delta X_{i,t} + \epsilon_{i,t}.
\end{aligned}
\tag{5}
$$

The variable $Network_i$ is the indicator variable for whether or not worker i had a former coworker who worked in the main construction sector. About 22% of the workers in the analysis sample have a former coworker who worked in the main construction sector during 1990–98. I include all the corresponding double interactions and indicators in $X_{i,t}$. I also include the same control variables as in Equation 2. I do not detrend the coefficients and confidence intervals of Equation 5. Similar to the triple difference specification of Equation 4, Equation 5 has the additional advantage over the simple DiD specification of filtering out common shocks in the low-wage and high-wage labor markets. Because Equation 5 compares groups of workers within outside option industries, it has the further advantage over the triple difference specification of Equation 4 of filtering out linear trends. Nevertheless, heterogeneity analyses of this form should not be over-interpreted as causal effects, but rather as suggestive evidence, because they introduce biases into the estimation, which are evident, for example, in statistically significant pre-trends: workers with coworker networks differ along unobserved dimensions from workers without such coworkers.

Figure 7 illustrates the results. For wages, I find that treated workers with a former coworker in the main construction sector during 1990–98 have a 2% higher wage growth on average compared to treated workers without such a former coworker in 1996. For establishment changes, I find a sizable statistically significant effect, showing that treated workers with a former coworker in the main construction sector during 1990–98 were 4.1 percentage points more likely to change their establishment right at the year of the public announcement of the minimum wage in 1996.

Worker Mobility vs. Establishment Wage Adjustments

The wage spillover effects can result from workers remaining with their employer and receiving a raise, or alternatively from workers changing employers and receiving a higher wage in their new position. An indication of the latter mechanism is that job-to-job changes increase at the same time as wage growth picks up (Figures 2 and 3).

In Figure 8, I compare the wage growth of workers who stayed with their establishment in the 1994–98 post-period to those who switched at least once in the same period. By using sub-samples for stayers and switchers, i.e. comparing stayers to stayers and switchers to switchers over time, this approach should alleviate concerns that switchers generally have higher wage growth than stayers. I find that the

[31] This sub-sample followed a similar data preparation as in Section 3.2. See also Appendix C.

wage spillover effects were driven by workers who changed establishments, not by those who stayed with their establishment. Moreover, this analysis allows me to rule out the possibility that bargaining within the existing employment relationship drove the wage spillover effects.

To further confirm this result from the establishment perspective, I estimate a DiD specification at the establishment level in Appendix B (I collapse the data to the establishment-year level). Here, I compare establishments with higher shares of treated employees to establishments with lower shares of treated employees within a set of outside option industries (for details see Appendix Sections B.1 and B.2). I find no wage effects for establishments with higher exposure, again suggesting that these establishments did not increase their wages. Thus, it is not establishment behavior that explains the spillover effects, but worker behavior.

Direction of Worker Flows

It would be reasonable to expect that workers would direct their search and job moves to the sector that introduced the minimum wage. In Figure 9, I use the probability to move to the main construction sector as the outcome variable to test this prediction. If anything, I only find a very small increase in moves to the main construction sector, but not large enough to explain the increase in job-to-job transitions of Figure 3.[32]

Where did these workers move to, if not into main construction? In Figure 10 panel (a), I show that treated workers moved out of their 1-digit industry. However, in panel (b) of Figure 10, I show that those who moved out of their 1-digit industry in the post-announcement period did not direct their search on a particular 1-digit industry (see the white bars of the histogram for the control group for comparison).[33] Rather, the choice of industry was dispersed. Thus, directed search towards specific 1-digit industries cannot explain the spillover effects in this paper.

Instead, I show that treated workers directed their search more generally toward better-paying employers. Specifically, I follow the approach in Dustmann et al. (2022). I define the change in the establishment j average wage or AKM establishment effect for worker i as $q^{l=t}_{j(i,t+2)} - q^{l=t}_{j(i,t)}$, where $q^{l}_{j(i,t+2)}$ denotes the time l characteristics of establishment j at which worker i is employed in year $t+2$. Thus, I measure the establishment average wage or AKM establishment effect in the baseline period t in both periods. For workers who remain employed at their baseline establishment from t to $t+2$, this measure of establishment quality is zero by construction. Using this approach, I make sure that any change in establishment average wage or AKM establishment effect reflects compositional changes only and not improvements in the quality of establishments over time.[34]

In the first panel of Figure 11, I show the detrended results for the change in average establishment (daily) imputed wages.[35] I find that treated workers in outside option industries had a higher likelihood of switching to establishments which pay a higher average wage to their workforce in the post-period. Specifically, treated workers in outside option industries switched to establishments that on average have a 1.6% higher mean wage than their previous establishment in 1994–96.

[32]In Appendix Figure A.4, I exclude moves to the main construction sector from the baseline DiD specifications and find almost no change in coefficients for the wage spillover and establishment change effects.

[33]Note that the main construction sector is not included in these 1-digit industries and that construction refers only to the ancillary construction industry. Moves to the main construction sector cannot explain the increase in establishment changes (see Figure 9).

[34]For an explanation of the AKM effects and the data I use, see Appendix E.2.

[35]Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. In comparison to the censored wage variable, the imputed wage variable has the benefit that it can more accurately represent job-to-job transitions to establishments with better workforce composition. For details on the imputation procedure see Ganzer et al. (2022).

In the second panel of Figure 11, I show the detrended difference-in-differences results using the change in AKM establishment fixed effects as the outcome variable. While a negative coefficient would indicate that workers moved to establishments with a lower pay premium to the same worker type, a positive coefficient indicates that workers moved to establishments with a higher pay premium to the same worker type. I find that treated workers in outside option industries moved to establishments that have a higher AKM establishment fixed effect. Specifically, these workers switched to establishments that on average have a 1% higher establishment AKM fixed effect than their previous establishment in 1994–96.

Worker Experience

Knowledge of outside options in the labor market varies among different types of workers. Specifically, more experienced workers have potentially already identified and pursued their desired career paths, whereas workers with less experience have not yet fully explored their potential outside options and are more susceptible to changes in these options.

To test this hypothesis, in Figure 12 I estimate the difference-in-differences specification of Equation 2 using the wage growth outcome separately for sub-samples of workers with 0 to 5 years, 5 to 10 years, or more than 10 years of labor market experience. Because I detect statistically significant linear trends within the different sub-samples, I detrend the coefficients and confidence intervals. As for Figure 8, by using sub-samples for different levels of experience, this approach should alleviate concerns that workers with low labor market experience generally have higher wage growth than workers with high labor market experience. I find larger spillover effects for workers with little labor market experience, while workers with more labor market experience had smaller spillover effects. Thus, the results suggest that workers who are more likely to have less knowledge about their outside options also experienced, on average, higher wage spillover effects relative to their better-informed counterparts following the publicly discussed and announced introduction of the minimum wage in the main construction sector.

Taking stock, I show in this section that workers with former coworkers in the main construction sector experienced higher wage growth and job transitions, suggesting that network effects were important for the spillovers. Moreover, these spillovers were primarily driven by workers changing establishments rather than by establishments raising wages. Workers do not direct their job search towards the main construction sector or any other specific industry, but rather towards better-paying employers. Finally, the results show that less experienced workers, who are likely to have less knowledge about their outside options, experienced larger wage spillovers. I discuss and rationalize these results in the next section.

6 Discussion

In this section, I discuss the results through the lens of two different models of monopsonistic competition in the labor market. The central contribution of this paper is to provide empirical evidence of the existence of horizontal spillovers from minimum wages. The models presented here are only meant to serve as an explanatory approach for the results found and to show that models with monopsonistic competition and labor market frictions can provide good explanatory approaches. However, the aim here is not to clearly reject and discriminate between certain models; other contexts are better suited for that.

A prominent model to explain spillovers between different employers involves strategic interaction, in which firms internalize the wage setting behavior of their competitors (e.g. in Staiger et al., 2010).

However, this model is ex-ante unlikely to apply here, as the minimum wage in the main construction sector did not directly affect wages in this sector in West Germany (see Appendix C), most likely due to the high coverage of firms with collective bargaining agreements that set wages higher than the minimum wage (see Section 2). Using a simplified version of the models of Bhaskar and To (1999); Bhaskar et al. (2002), which in turn build on the spatial model of Salop (1979), I provide suggestive evidence against strategic complementarity as a mechanism for the spillovers in West Germany. These models suggest that geographically distant sectors have greater autonomy in setting wages than proximate sectors. Thus, wage spillovers should be higher in regions with a greater presence of the main construction sector, where outside option industries are closer to the main construction sector. The model further predicts that in regions where the main construction sector has a higher bite, more establishments will adjust wages upward, leading to wage increases in outside option industries, whereas in regions with a low bite, minimal wage adjustments will occur in either sector. Note that because I use employment flows to determine outside option industries, these industries are already "close" to the main construction sector in terms of task similarity, skill transferability, and possibly other non-pecuniary characteristics through revealed preference. Therefore, I assume that only geographic proximity is relevant for my empirical tests of this model to be conclusive about strategic complementarity (as in Staiger et al., 2010). For modeling details, I refer the interested reader to Appendix D.1.[36] Table 5 shows that the wage spillovers did not correlate with geographic proximity to the main construction sector (proxied by terciles of the distribution of the share of the main construction sector among LMRs)[37], nor were they higher in regions with a greater minimum wage bite in West Germany.[38] More generally, models of strategic interaction postulate that establishments respond to wage-setting changes in other establishments. In this context, however, the spillover effects were driven by the behavior of workers, not the behavior of establishments, as I show in Section 5.3.

Nevertheless, I argue that models of strategic interaction among employers are more relevant in contexts where the minimum wage has a significant bite and has a direct effect on wages within the minimum wage sector. In the Appendix C I show that, in contrast to the effects in West Germany, the main construction sector minimum wage had direct effects on wages for workers, establishments, and regions in East Germany. Therefore, I repeat the tests of the strategic complementarity model in Table 6 for East Germany and find evidence consistent with that model. Specifically, in East Germany, wage spillovers increase in LMRs with a higher bite of the minimum wage.[39]

I briefly sketch a model that provides an alternative explanation for the pattern of spillovers in West Germany based on information frictions. Two sources of information could have informed workers about their outside options in this context. First, as I show in Section 5.3, coworker networks are one source of information, and indeed I find that workers who had former coworkers in the main construction sector around the time of the public announcement of the minimum wage experienced larger spillover effects (Figure 7). Second, the extensive media coverage and intense wage negotiations suggest that information frictions about appropriate wages for workers with tasks and skills similar to those in the

[36] In a more sophisticated version of these models, Berger et al. (2022) show that spillovers from changes in a firm's or sector's wage setting can occur whenever the wage increase also increases its employment and market share. However, in the context of this paper, the minimum wage did not, on average, affect the employment of establishments in the main construction sector in West Germany (see Appendix C).

[37] See Appendix D.2. I assume that the share of the main construction sector in a labor market region (LMR) is negatively correlated with the distance to its competitors in the LMR.

[38] To test this prediction, I use the bite of the main construction sector minimum wage, calculated for each LMR using the pre-period (see Appendix C.2). I standardize the bite measure across LMRs, weighted by the number of employees in each LMR, to have a mean of 0 and a standard deviation of 1.

[39] Appendix Table A.6 (A.7) illustrates the full table with the partially treated group for West (East) Germany.

main construction sector may have been significantly reduced, making this a likely mechanism for the spillover effects. In other words, for workers in jobs similar to those in the main construction sector, earning a wage below a minimum that is discussed as unfair could induce workers to search for better paying jobs. However, I cannot provide direct evidence for this source of information.

I apply the theoretical model of Jäger et al. (2024) to my context and derive testable predictions from it. I present the main components of the model relevant to my context in the Appendix E.1 and refer the interested reader to Jäger et al. (2024) for details. In this model, workers can have information costs that lead to biased beliefs about their outside options in the labor market, no incentive to search for jobs, and a marked-down wage while staying in low-paying firms. Three pieces of evidence are particularly consistent with an information shock story. First, an increase in wage spillovers and job-to-job transitions right around the year of the public discussion and announcement of the minimum wage in 1996 and before its introduction in 1997 (Figures 2 and 3). Second, the model predicts that through the publicly discussed and announced introduction of the minimum wage in the main construction sector, treated workers in outside option industries learn what wages they could earn in the labor market, not just in the main construction sector but also in general, explaining their broader job search behavior.[40] They learn that they are working in a low-wage establishment that pays them a marked-down wage with a lower wage-tenure profile, and as a result they move to a better-paying establishment (Figure 11). Third, the model in Jäger et al. (2024) distinguishes between workers with high information costs (amateurs) and workers with no information costs (experts). Only amateurs should be affected by the information shock. Experts were already aware of wages above the minimum wage in establishments covered by collective bargaining agreements in the main construction sector (see Section 2). As a result, the public discussion and announcement of the minimum wage, which is below the entry level wage in establishments covered by collective bargaining agreements, should not have had any effect on the experts. Assuming that workers with less labor market experience also have less information about possible outside options in the labor market than workers with more labor market experience, I find evidence for this prediction in Figure 12.

The results of this paper also have implications for how wages are set in the low-wage labor market. In particular, wage bargaining models assume that workers can renegotiate wages with their current employer in the presence of new outside options or information about their outside options. In contrast, wage posting models postulate take-it-or-leave-it offers by employers, in which workers must leave their current employer to obtain higher wages when outside options or information about outside options change (see e.g. Hall and Krueger, 2012). Because I find that the wage spillover results are driven by workers who switch establishments rather than stay and receive a wage increase (Figure 8) and that these workers reallocate to better paying employers (Figure 11), the results of this paper suggest that wage posting is the more dominant mode of wage determination for low-wage workers. This finding is consistent with other studies that find wage posting to be more important for the low-wage labor market (e.g. Cahuc et al., 2006; Lachowska et al., 2022).

7 Conclusion

Minimum wages targeted at specific regions or sectors remain an important policy tool, but evidence on their effects beyond the targeted area remains scarce. This paper aims to address this issue by

[40] Workers learn not only about wage setting in the main construction sector, but also about what constitutes unfair "dumping wages". For example, the term "dumping wages" was often used by unions in the context of the minimum wage debate to defame wage proposals by employers' associations (e.g. *Frankfurter Allgemeine Zeitung*, March 14, 1996, No. 63, p.15).

analyzing a unique context to isolate the horizontal wage spillover effects of a sectoral minimum wage. Specifically, I analyze the horizontal spillover effects of the minimum wage in the main construction sector in Germany on workers in similar jobs outside the targeted sector and find an increase in wages and worker reallocation. In addition, I find an increase in worker transitions to better paying employers and a resulting higher wage growth in the year of public announcement and discussion of the minimum wage.

A back-of-the-envelope calculation suggests that those exposed to the spillover effects earned on average 686 Euro more every year after the public discussion and announcement of the minimum wage than they would have earned without the public discussion and announcement.[41] If we take into account that sub-minimum wage workers earned an average of 20,838 Euro annually before the minimum wage was announced, this shows that the spillover effects have led to a substantial improvement in the income situation of low-wage employees. Moreover, because low-paying establishments are less productive than high-paying establishments (Abowd et al., 1999), the reallocation of employment from low-paying establishments to high-paying establishments may have increased the welfare of the economy as a whole.

The current German government is again increasingly thinking in the direction of generally binding collective bargaining agreements in order to set sectoral minimum wages. In the coalition agreement, the government parties agreed to tie public payments to compliance with a representative collective bargaining agreement for the respective sector (SPD et al., 2021). In this context, the current German government has already passed the *Gesundheitsversorgungsweiterentwicklungsgesetz* (Health Care Advancement Act), which will restrict public payments to care facilities that pay their employees according to collective bargaining agreements. In this paper, I have shown that publicly disclosed sectoral collective agreements can have a significant spillover effect on the low-wage labor market and thus have positive wage and reallocation effects beyond the boundaries of the sector actually affected.

[41] On average, sub-minimum wage workers in my sample earned 57.09 Euro daily before the public announcement of the minimum wage (Table 2). Two-year wage growth was 12% before the public announcement. Thus, daily wages grew by an average of 6.85 Euro every two years. After the public announcement, the daily wage grew by 15.3% every two years and thus by an average of 8.73 Euro every two years. For a continuously employed person this means on average (1.88 Euro $\times$ 365 days) 686 Euro more every year.

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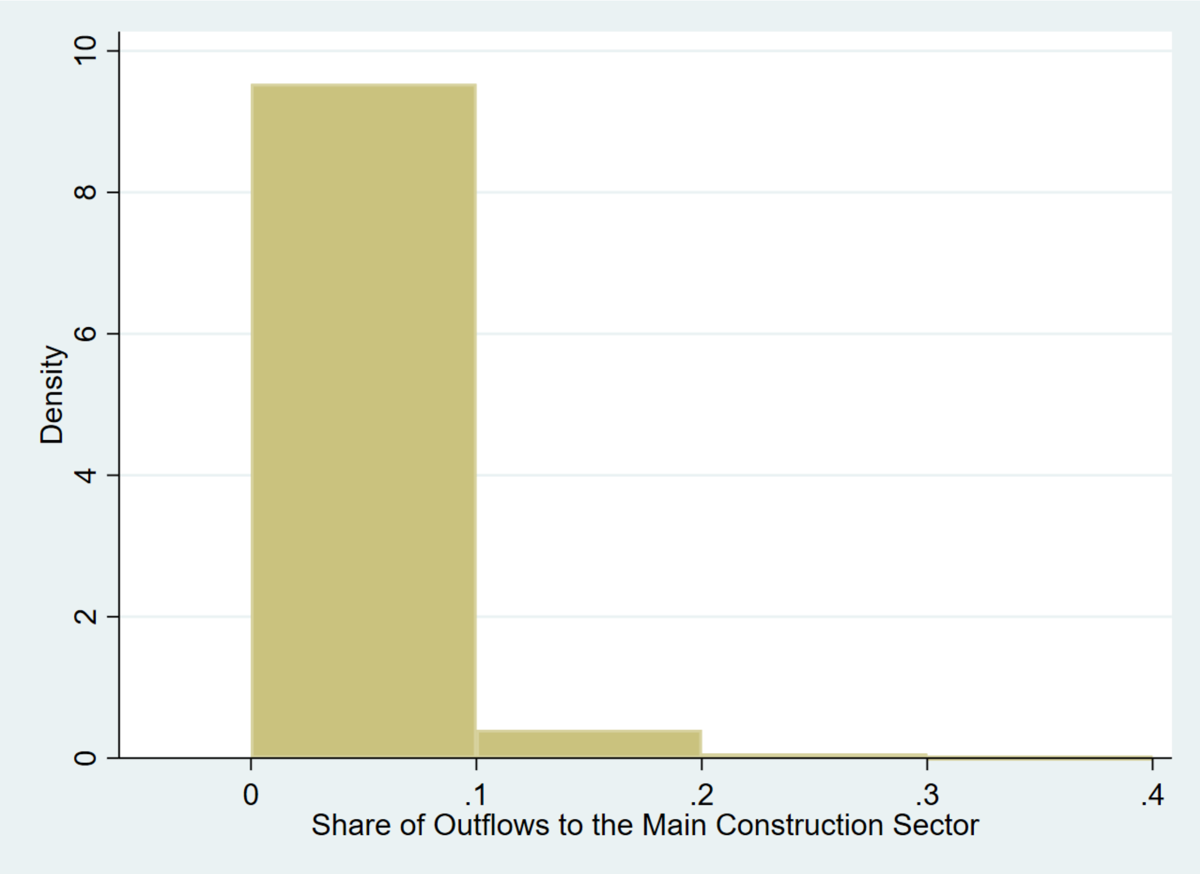
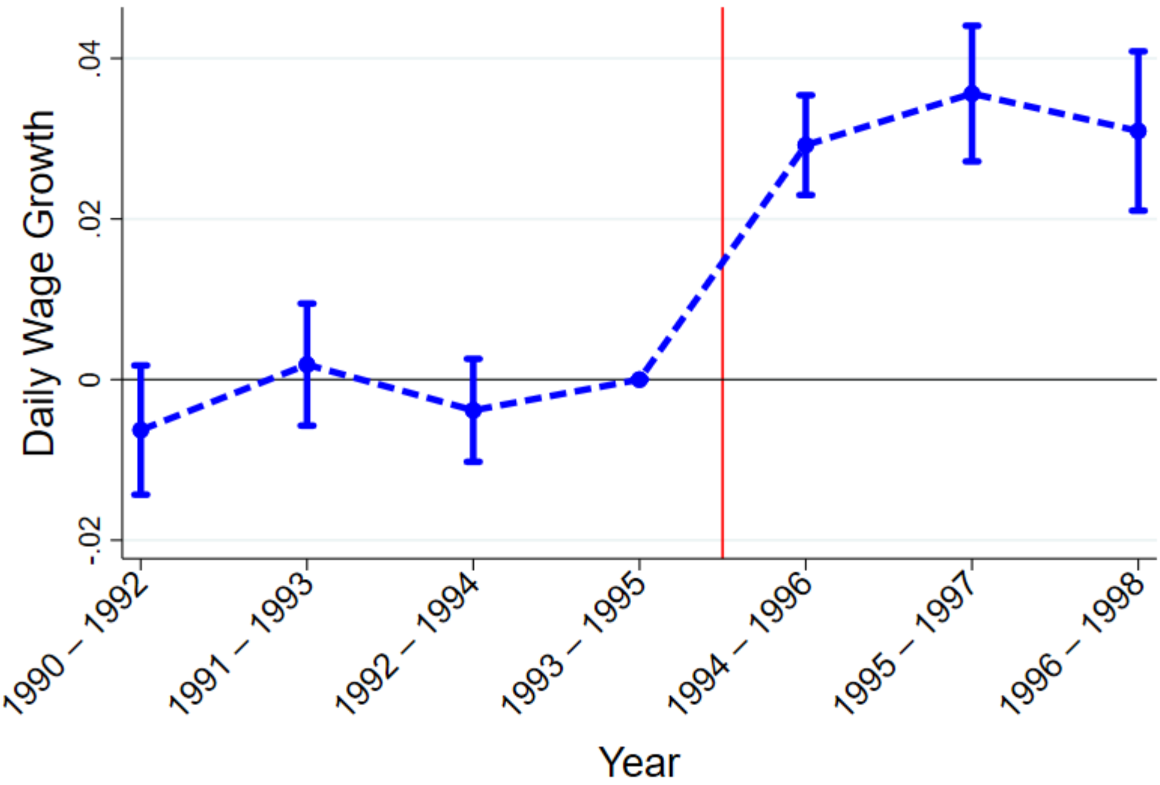
Figure 1: Density of the Share of Outflows to the Main Construction Sector by 3-digit Industries



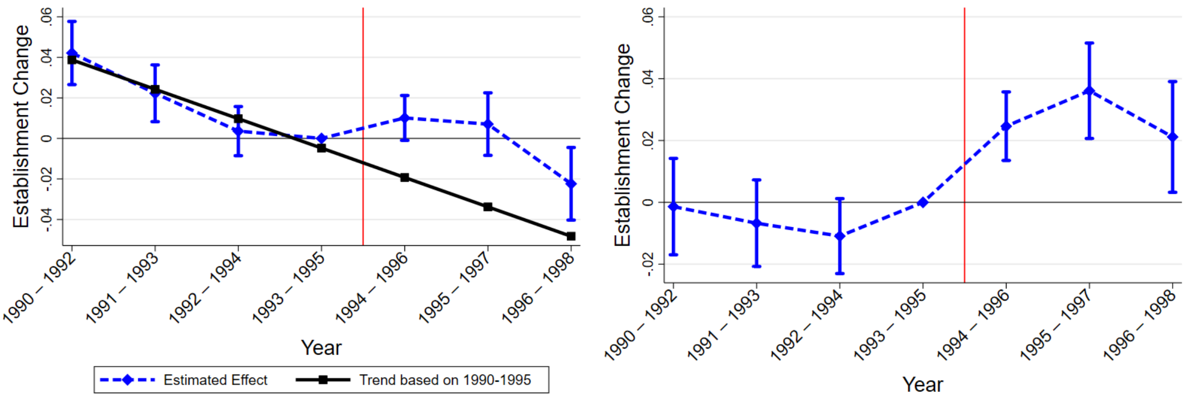
Notes: For this figure, I only keep observations from the period 1992–95 in Germany and drop all observations with missing two-year wage growth or treatment assignment. The figure shows the share of outflows to the main construction sector by 3-digit industries weighted by the number of workers in each industry from 1992–95. **Source:** SIEED and BHP 1992–95. Authors' calculations.

Figure 2: Wage Spillover Effects of the Main Construction Sector Minimum Wage



Notes: This figure illustrates the results of the difference-in-differences specification for treated versus control group workers, with the two-year change in log daily wages as the outcome (see Equation 2). I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Column 1 of Appendix Table A.5 illustrates the results in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Authors' calculations.

Figure 3: Establishment Change Effects of the Main Construction Sector Minimum Wage

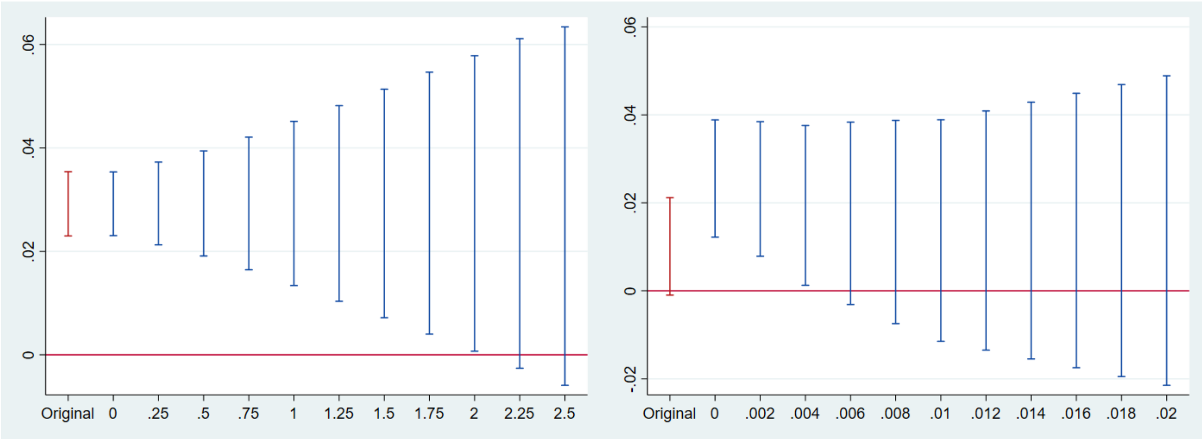


(a) Establishment Change

(b) Detrended Establishment Change

Notes: This figure illustrates the results of the difference-in-differences specification for treated versus control group workers, with the two-year change in establishments as the outcome (see Equation 2). I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. In panel (a), I present the results with linear time trends estimated for the 1990–95 pre-announcement period and extrapolated to the post-announcement periods. In panel (b), I use the estimated linear time trend from (a) to present the detrended coefficients and confidence intervals. Column 2 of Appendix Table A.5 illustrates the results of panel (a) in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Authors' calculations.

Figure 4: Robustness Check on the Linear Pre-Trend Assumption

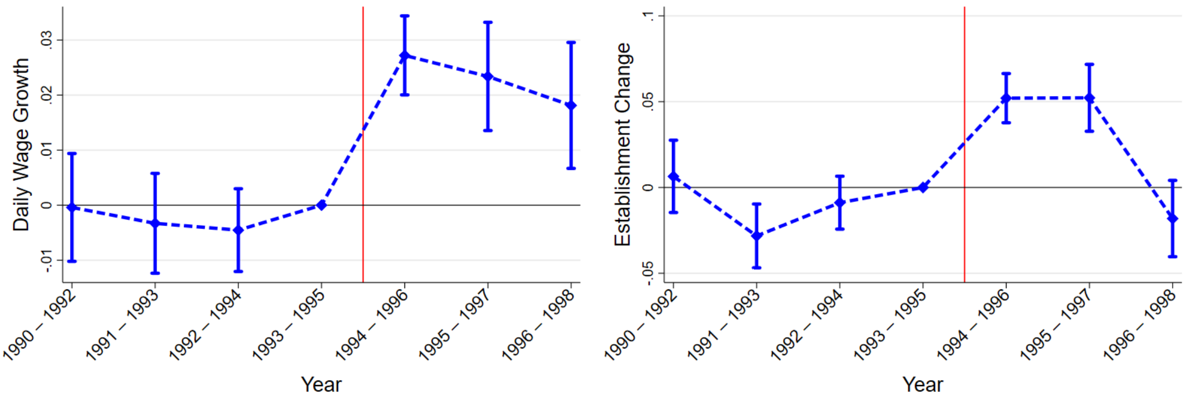


(a) Daily Wage Growth

(b) Establishment Change

Notes: Following the procedure in Rambachan and Roth (2023), this figure illustrates the first post-period coefficient of the change in wage growth (panel (a)) and change in establishments (panel (b)) (see Equation 2) and plots alternative confidence intervals that allow for deviations from parallel linear trends in the pre-period. On the x-axis, the confidence intervals in "Original" of panel (a) (panel (b)) correspond to the coefficient in 1994–96 of Figure 2 (3 panel (a)). In panel (a), the confidence intervals correspond to the bounds of the maximum post-treatment violation of parallel trends between consecutive periods by the maximum pre-treatment violation of parallel trends multiplied by the value on the x-axis. For example, the confidence intervals at "1" on the x-axis bound the worst case post-treatment difference in trends by the corresponding maximum in the pre-treatment period. In contrast to panel (a), the confidence intervals in panel (b) correspond to different violations of the linear group-specific time trend assumption. The confidence intervals at "0" of the x-axis correspond to the assumption of exact linear trends that are subtracted from the first post-period coefficient as in panel (b) of Figure 3. The confidence intervals at "0.002" on the x-axis correspond to the case when the slope of the linear trend in the post-period increases by 0.002 compared to the pre-period. **Source:** SIEED and BHP. Authors' calculations.

Figure 5: Triple Differences: Wage and Establishment Change Effects of the Main Construction Sector Minimum Wage

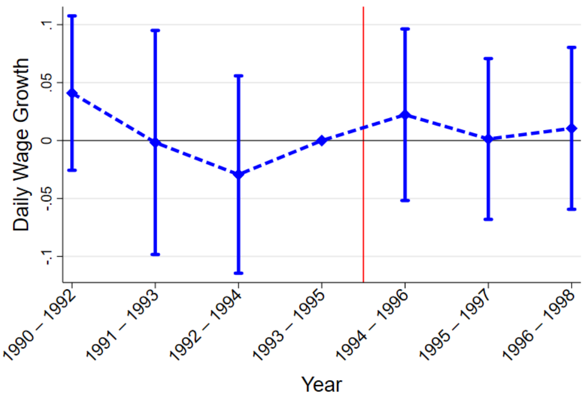


(a) Detrended Daily Wage Growth

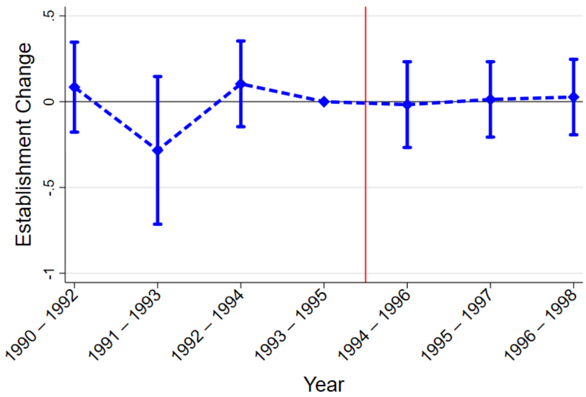
(b) Detrended Establishment Change

Notes: This figure illustrates the results of the triple differences specification for treated versus control group workers, with the two-year change in log daily wages (panel (a)) and change in establishments (panel (b)) as outcome variables (see Equation 4). The regressions use 656,665 worker-year observations with 133,502 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. In both panels, I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. **Source:** SIEED and BHP. Authors' calculations.

Figure 6: Placebo Test: Wage and Establishment Change Effects of the Main Construction Sector Minimum Wage on the Control Group



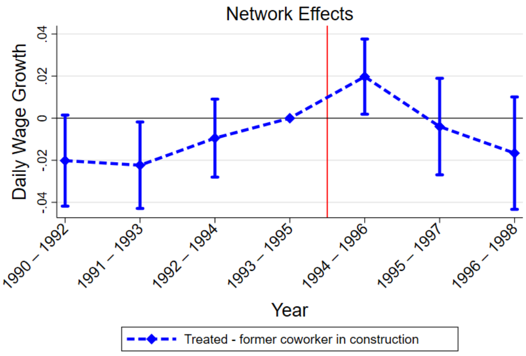
(a) Daily Wage Growth



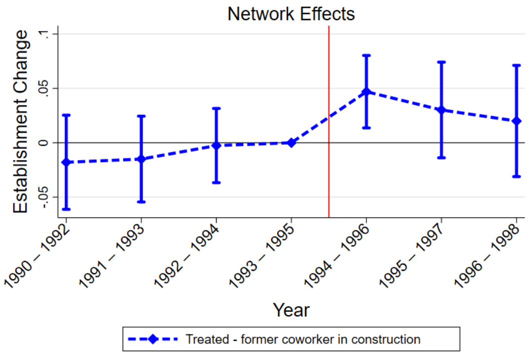
(b) Establishment Change

Notes: This figure illustrates the results of the difference-in-differences specification with the two-year change in log daily wages (panel (a)) and change in establishments (panel (b)) as outcome variables (see Equation 2). The regressions use 219,243 worker-year observations with 45,999 workers. Here, I define the partially treated group as workers with hourly wages that amount up to $8.69 + 80\% \leq h_{i,t} < 8.69 + 160\%$ and a second control group with hourly wages that amount up to $8.69+160\% \leq h_{i,t} < 8.69+200\%$. The variable $Treated_{i,t}$ now takes the value 0 if worker i is in the second control group in year t and 1 if she is in the base control group. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

Figure 7: Network Effects: Former Coworkers in the Main Construction Sector



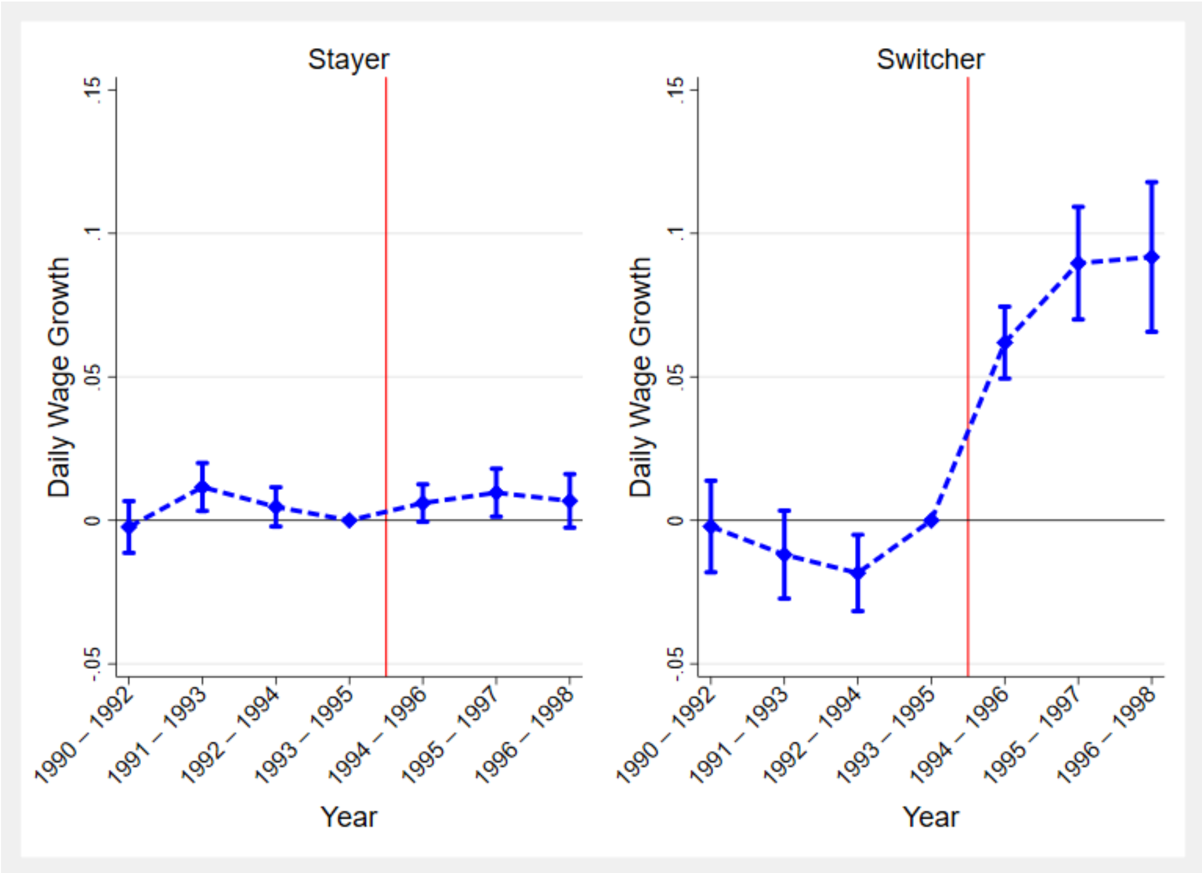
(a) Daily Wage: Former Coworker in Main Construction 1990–98



(b) Establishment Change: Former Coworker in Main Construction 1990–98

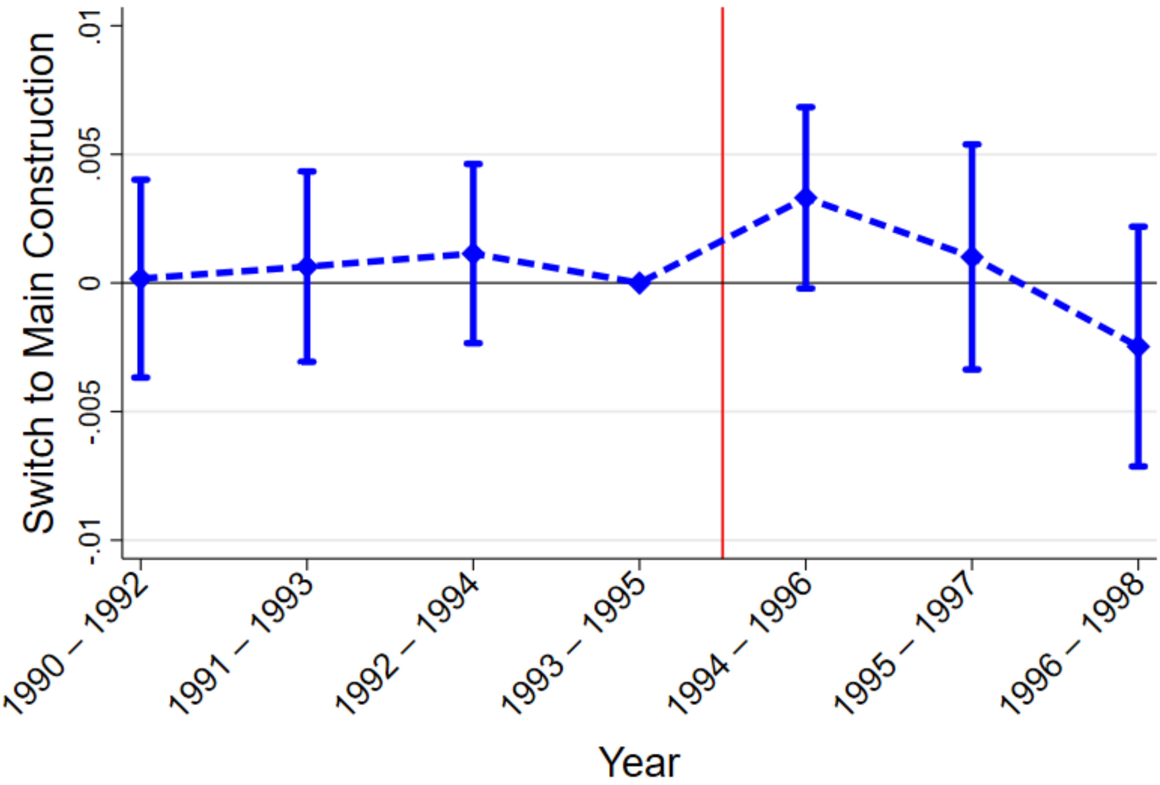
Notes: This figure illustrates the results of the triple interaction term of Equation 5 for treated versus control group workers, with the two-year change in log daily wages (panel (a)) and change in establishments (panel (b)) as outcome variables. I use 95% confidence intervals with standard errors clustered at the worker level. The sample is the same as in Figures 2 and 3 (see also Table A.5). Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects, worker fixed effects, and all respective interaction terms and single indicators of $Treated_{i,t}$ ($Partial_{i,t}$), $Network_i$, $Year_t$. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure 8: Wage Spillover Effects for Stayers vs. Switchers



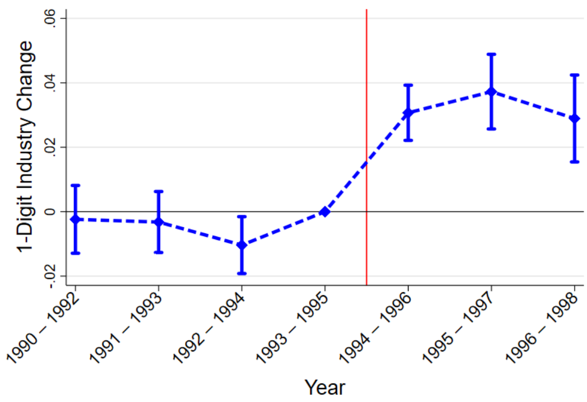
Notes: This figure shows the results of two difference-in-differences specifications for treated versus control group workers, using the two-year change in log daily wages as the outcome (see Equation 2). I define Stayers as workers who stayed within the same establishment during the 1994–98 period. Switchers are workers who changed establishments at least once from t to $t + 2$ during 1994–98. For the left panel, I use a sub-sample of Stayers with 192,303 worker-year observations and 36,991 workers. For the right panel, I use a sub-sample of Switchers with 100,259 worker-year observations and 23,319 workers. Because wage growth follows a nearly linear trend for workers who changed establishments, as also shown in Figure 3, I present detrended coefficients and confidence intervals for switchers and (for comparison also) for stayers by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure 9: Difference-in-Differences: Moving to the Main Construction Sector

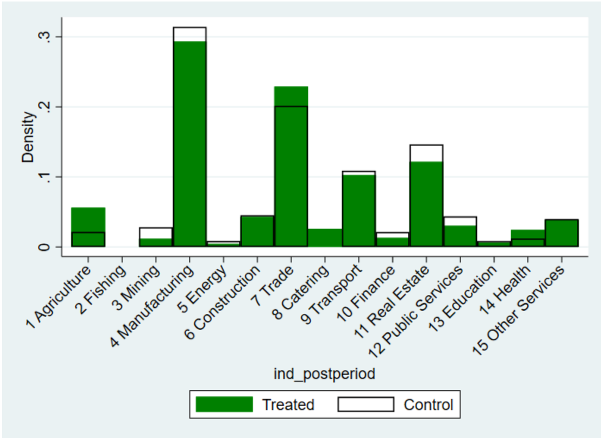


Notes: This figure shows the results of a difference-in-differences specification for treated versus control group workers, using the probability to move to the main construction sector as the outcome (see Equation 2). Specifically, the outcome variable takes the value 1 if the worker switched from an outside option industry in t to the main construction sector in $t+2$ and 0 if not. The regression uses 292,562 worker-year observations with 60,310 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. **Source:** SIEED and BHP. Author's calculations.

Figure 10: 1-Digit Industry Change and Distribution of Destination Industries



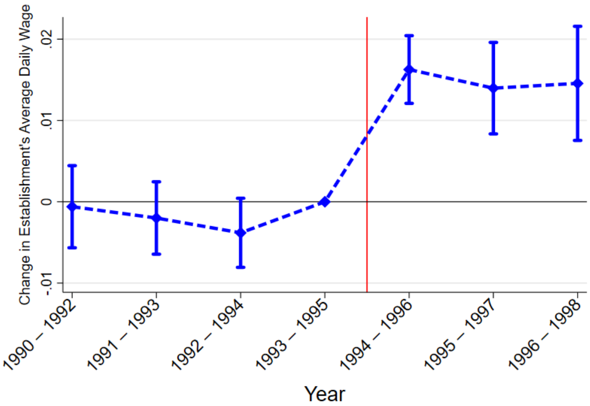
(a) Detrended 1-Digit Industry Change



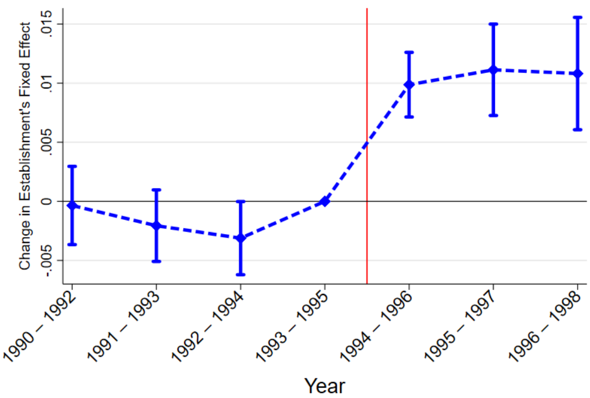
(b) Histogram: Post-Announcement Industry for Switchers Only

Notes: This figure illustrates the results of a difference-in-differences specification for treated versus control group workers, with the two-year change in 1-digit industry as the outcome variable in panel (a) (see Equation 2). The regression uses 292,562 worker-year observations with 60,310 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. Panel (b) presents a histogram of the post-announcement 1-digit destination industries for those who switched out of their industry. Specifically, for the histogram, I only keep individuals who worked in outside option industries in the 1990–95 pre-period, and made at least one 1-digit industry transition in the post-period. Note that the main construction sector is not included in these 1-digit industries and that construction refers only to the ancillary construction industry. The green bars show the distribution of industries for treated workers and for comparison, the white bars show the distribution of industries for control workers. **Source:** SIEED and BHP. Authors' calculations.

Figure 11: Difference-in-Differences: Reallocation to Higher-Paying Establishments



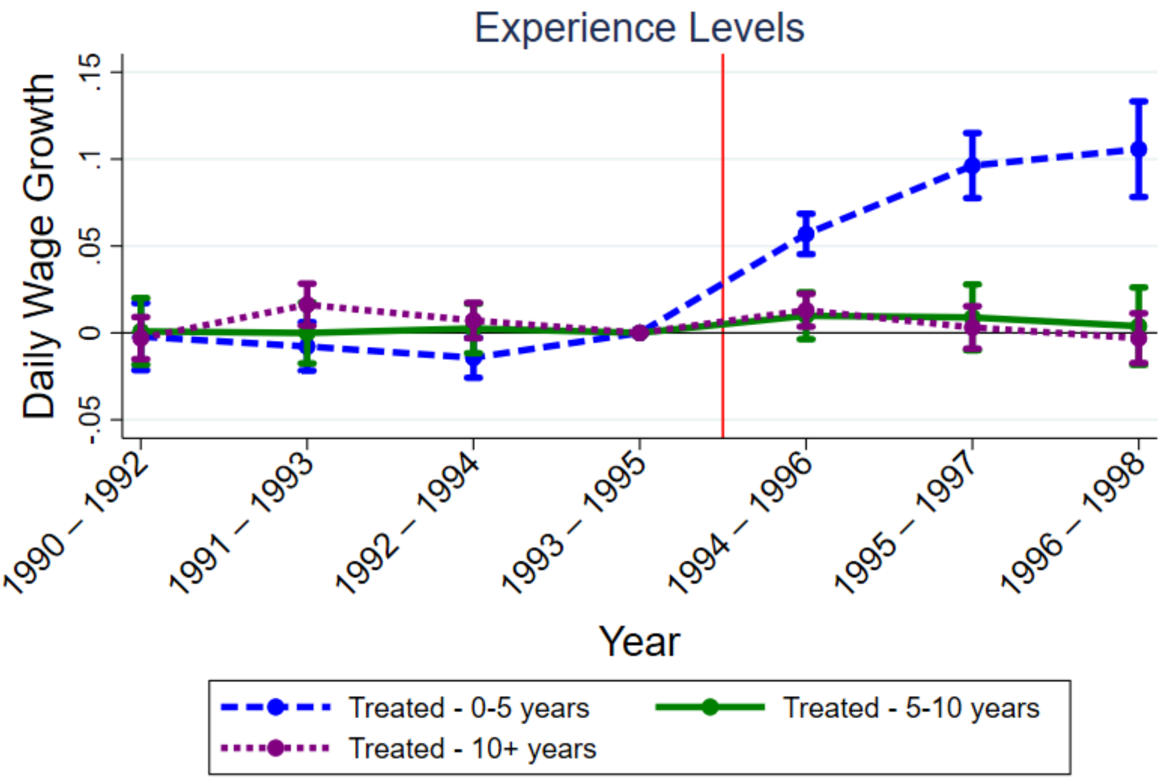
(a) Detrended Change in Establishment's Average Daily Wage



(b) Detrended Change in Establishment's Fixed Effect

Notes: This figure shows the results of two difference-in-differences specifications for treated versus control group workers, using different outcome variables (see Equation 2). I use 95% confidence intervals. In the first panel, I use the change in log establishment average imputed wages as the outcome variable. Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. The regression uses 265,036 worker-year observations with 56,858 workers. In the second panel, I use the change in establishment AKM fixed effects as the outcome variable. The regression uses 257,450 worker-year observations with 55,870 workers. I measure establishment quality in both specifications in t. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. **Source:** SIEED and BHP. Author's calculations.

Figure 12: Difference-in-Differences: Heterogeneity in Wage Spillover Effects



Notes: This figure illustrates the results of the difference-in-differences specification for treated versus control group workers, with the two-year change in log daily wages as the outcome (see Equation 2). I use 95% confidence intervals. I present the results separately (each line) for sub-samples of workers with 0 to 5 years (73,304 worker-year observations with 21,975 workers), 5 to 10 years (69,481 worker-year observations with 21,418 workers), and 10+ years of labor market experience (134,412 worker-year observations with 28,628 workers). Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. **Source:** SIEED and BHP. Authors' calculations.

Table 1: Sectoral Minimum Wages in Germany

Sector	First MW	Hourly Wage (in Euro)
Main Construction	01/1997	West (incl. Berlin) 8.69; East 8.00
Electrical Trade	06/1997	West 8.03; East (incl. Berlin) 6.41
Roofing	10/1997	West (incl. Berlin) 8.18; East 7.74
Painting & Varnishing	12/2003	West (incl. Berlin) 7.69; East 7.00
Commercial Cleaning	07/2007	West (incl. Berlin) 7.87; East 6.36
Waste Removal	01/2010	8.02
Nursing Care	08/2010	West (incl. Berlin) 8.50; East 7.50
Security	06/2011	Federal states: ranges from 6.53 to 8.60
Temporary Work	01/2012	West 7.89; East (incl. Berlin) 7.01
Scaffolding	08/2013	10.00
Stonemasonry	10/2013	West (incl. Berlin) 11.00; East 10.13
Hairdressing	11/2013	West 7.5; East (incl. Berlin) 6.5
Chimney Sweeping	04/2014	12.78
Slaughtering & Meat Processing	08/2014	7.75
Textile & Clothing	01/2015	West 8.5; East (incl. Berlin) 7.5
Agriculture, Forestry & Gardening	01/2015	West 7.4; East (incl. Berlin) 7.2

Table 2: Descriptives for Individuals in Outside Option Industries (1990–95)

	Treated Group		**Partially Treated Group**		**Control Group**	
No. of observations	73,574		182,090		137,285	
Share	18.72		46.34		34.94	
Averages						
Daily wage	57.09	(14.03)	87.39	(9.23)	111.30	(10.23)
Log (daily) wage	4.01	(0.29)	4.46	(0.11)	4.71	(0.09)
Log (daily) two-year wage growth	0.12	(0.27)	0.02	(0.16)	-0.01	(0.14)
Shares within group (in percent)						
Women	46.01		17.01		11.27	
Non-German nationality	10.41		10.70		9.00	
By age						
18-25 years old	34.44		22.79		8.47	
26-35 years old	34.97		45.02		46.38	
36-45 years old	18.85		19.80		27.50	
46-55 years old	9.56		10.03		14.49	
56-65 years old	2.18		2.36		3.17	
By education						
No vocational training	19.39		12.88		8.77	
Vocational training	78.12		84.33		84.51	
University or university of applied sciences	1.96		2.48		6.45	
Missing education	0.53		0.30		0.28	
By industry						
Agriculture and Forestry	25.44		9.45		4.17	
Mining	1.38		3.89		7.08	
Manufacturing	39.75		59.19		58.14	
Construction	7.19		8.54		5.73	
Real Estate and Housing	22.10		16.08		20.64	
Other Services	1.28		2.52		4.10	
Missing industry	2.85		0.33		0.14	
By plant size						
Very small (1-4 workers)	31.71		12.20		6.94	
Small (5-19 workers)	32.79		30.34		22.71	
Medium (20-249 workers)	29.86		43.32		47.94	
Large (250-999 workers)	4.94		11.01		17.26	
Very large (1000+ workers)	0.70		3.13		5.15	
By region type						

Continued on next page

Table 2 – continued from previous page

	Treated Group	Partially Treated Group	Control Group
District-free cities	20.24	20.31	24.24
Urban districts	42.53	44.78	49.38
Rural districts, some densely populated areas	21.00	19.05	14.91
Rural districts, sparsely populated	16.23	15.85	11.47

Notes: Observations are worker-year combinations. Standard deviation in parentheses. The groups are defined by using the nominal hourly wage of a worker at year t. Daily wages are deflated using the consumer price index of the Federal Statistical Office. The table only includes workers in the outside option industries in West Germany for the pre-announcement period of 1990–95.
Source: SIEED and BHP, 1990–1995. Authors' calculations.

Table 3: Difference-in-Differences: Pre- vs. Post-Period Specifications

	2-year wage growth	Job-to-job
Treated x Post	0.033***	0.031***
	(0.003)	(0.007)
Partial x Post	0.011***	0.019***
	(0.001)	(0.005)
No. of observations	292,562	292,562
No. of workers	60,310	60,310
Year fixed effects	yes	yes
3-digit industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes
Group-specific time trends	no	yes

Notes: Standard errors in parentheses. The table shows specifications of Equation 3 with different outcome variables. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

Table 4: Difference-in-Differences: Robustness Checks

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	Region + Industry + Occupation shocks	No closing plants	Tight Definition	Broad Definition
Panel A: Wages								
Treated x Post	0.033***	0.034***	0.031***	0.032***	0.033***	0.030***	0.033***	0.044***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Partial x Post	0.011***	0.012***	0.011***	0.012***	0.012***	0.011***	0.045***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
No. of observations	292,562	288,869	294,149	290,436	290,436	290,902	239,855	347,146
No. of workers	60,310	59,574	60,592	59,852	59,852	60,087	52,588	68,057
Panel B: Job-to-job								
Treated x Post	0.031***	0.028***	0.033***	0.033***	0.029***	0.030***	0.022***	0.023***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
Partial x Post	0.019***	0.021***	0.021***	0.021***	0.021***	0.019***	0.024***	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.005)
No. of observations	292,562	288,869	294,149	290,436	290,436	290,902	239,855	347,146
No. of workers	60,310	59,574	60,592	59,852	59,852	60,087	52,588	68,057
Year fixed effects	yes	no	no	no	no	yes	yes	yes
3-digit industry fixed effects	yes	yes	no	no	no	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
LMR x year fixed effects	no	yes	no	yes	yes	no	no	no
Industry x year fixed effects	no	no	yes	yes	yes	no	no	no
Occupation x year fixed effects	no	no	no	no	yes	no	no	no

Continued on next page

Table 4 – continued from previous page

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	Region + Industry + Occupation shocks	No closing plants	Tight Definition	Broad Definition

Notes: This table shows several robustness checks on the difference-in-differences estimation with the two-year change in log daily wages as the outcome variable in Panel A and the two-year change in job-to-job transition as the outcome variable in Panel B (see Equation 3). To account for group-specific linear time trends in job-to-job transitions, I interact the group indicators with the linear time trend $time_t$ in Panel B. Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification. In the second column, I add labor market region times year fixed effects. In the third column, I add 1-digit industry times year fixed effects to the baseline specification. In the fourth column, I combine labor market region times year fixed effects and industry times year fixed effects and add them to the baseline specification. In the fifth column, I additionally include 1-digit occupation times year fixed effects. In the sixth column, I use the baseline specification and drop all observations in establishments that are in their closing year. In the seventh column, I use a tight definition of the partially treated $(8.69 \leq h_{i,t} < 8.69 + 20\%)$ and control group $(8.69 + 20\% \leq h_{i,t} < 8.69 + 60\%)$. In the eight column, I use a broad definition of the partially treated $(8.69 \leq h_{i,t} < 8.69 + 60\%)$ and control group $(8.69 + 60\% \leq h_{i,t} < 8.69 + 120\%)$. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 5: Tests of Strategic Complementarity Model Predictions in West Germany

	Baseline	Tercile share	Bite
Treated x Post	0.033***	0.040***	0.033***
	(0.003)	(0.005)	(0.003)
Treated x Middle x Post		-0.021***	
		(0.007)	
Treated x High x Post		-0.004	
		(0.007)	
Treated x Bite x Post			-0.001
			(0.003)
No. of observations	292,562	288,869	286,871
No. of workers	60,310	59,574	59,171
Year fixed effects	yes	yes	yes
3-digit industry fixed effects	yes	yes	yes
Federal state fixed effects	yes	yes	yes
Region type fixed effects	yes	yes	yes
Worker fixed effects	yes	yes	yes
LMR fixed effects	no	yes	yes

Notes: Standard errors in parentheses. The table shows specifications using Equation 3 with the 2-year change in log (daily) wages as the outcome for West Germany. The first column shows the baseline estimation, also illustrated in Table 3. In the second column, I interact the baseline DiD interaction additionally with the terciles of the share of the main construction sector within a LMR. I also include the indicator variable for tercile as an additional control. ”Middle” indicates workers who work in a LMR in the middle tercile of the employment weighted distribution of shares of the main construction sector and ”High” indicates workers in the highest tercile of this distribution. In the third column, I interact the baseline DiD interaction additionally with the bite of the main construction sector minimum wage in each LMR. I calculate the bite as the share of employees who earned below the first minimum wage threshold in the pre-period within the main construction sector. I standardize the bite measure to have mean zero and standard deviation 1. All specifications include the baseline fixed effects: year, 3-digit industry, federal state, region type, and worker. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Source: SIEED and BHP. Authors’ calculations.

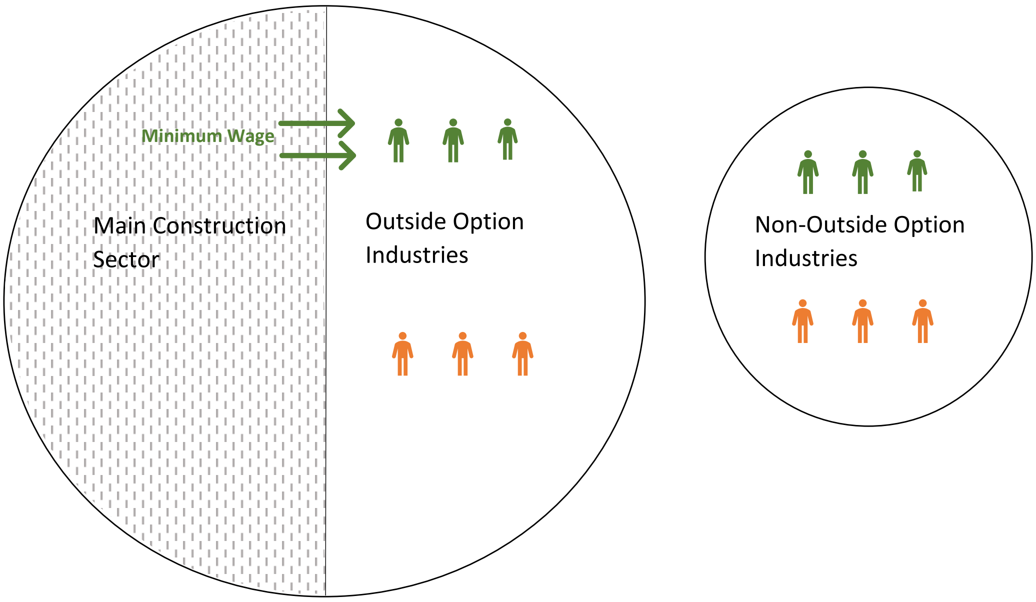
Table 6: Tests of Strategic Complementarity Model Predictions in East Germany

	Baseline	Tercile share	Bite
Treated x Post	0.032***	0.047***	0.034***
	(0.004)	(0.017)	(0.004)
Treated x Middle x Post		-0.020	
		(0.019)	
Treated x High x Post		-0.016	
		(0.018)	
Treated x Bite x Post			0.008*
			(0.004)
No. of observations	73,116	73,116	73,116
No. of workers	19,612	19,612	19,612
Year fixed effects	yes	yes	yes
3-digit industry fixed effects	yes	yes	yes
Federal state fixed effects	yes	yes	yes
Region type fixed effects	yes	yes	yes
Worker fixed effects	yes	yes	yes
LMR fixed effects	no	yes	yes

Notes: Standard errors in parentheses. The table shows specifications using Equation 3 with the 2-year change in log (daily) wages as the outcome for East Germany. The first column shows the baseline estimation, also illustrated in Table 3. In the second column, I interact the baseline DiD interaction additionally with the terciles of the share of the main construction sector within a LMR. I also include the indicator variable for tercile as an additional control. "Middle" indicates workers who work in a LMR in the middle tercile of the employment weighted distribution of shares of the main construction sector and "High" indicates workers in the highest tercile of this distribution. In the third column, I interact the baseline DiD interaction additionally with the bite of the main construction sector minimum wage in each LMR. I calculate the bite as the share of employees who earned below the first minimum wage threshold in the pre-period within the main construction sector. I standardize the bite measure to have mean zero and standard deviation 1. All specifications include the baseline fixed effects: year, 3-digit industry, federal state, region type, and worker. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

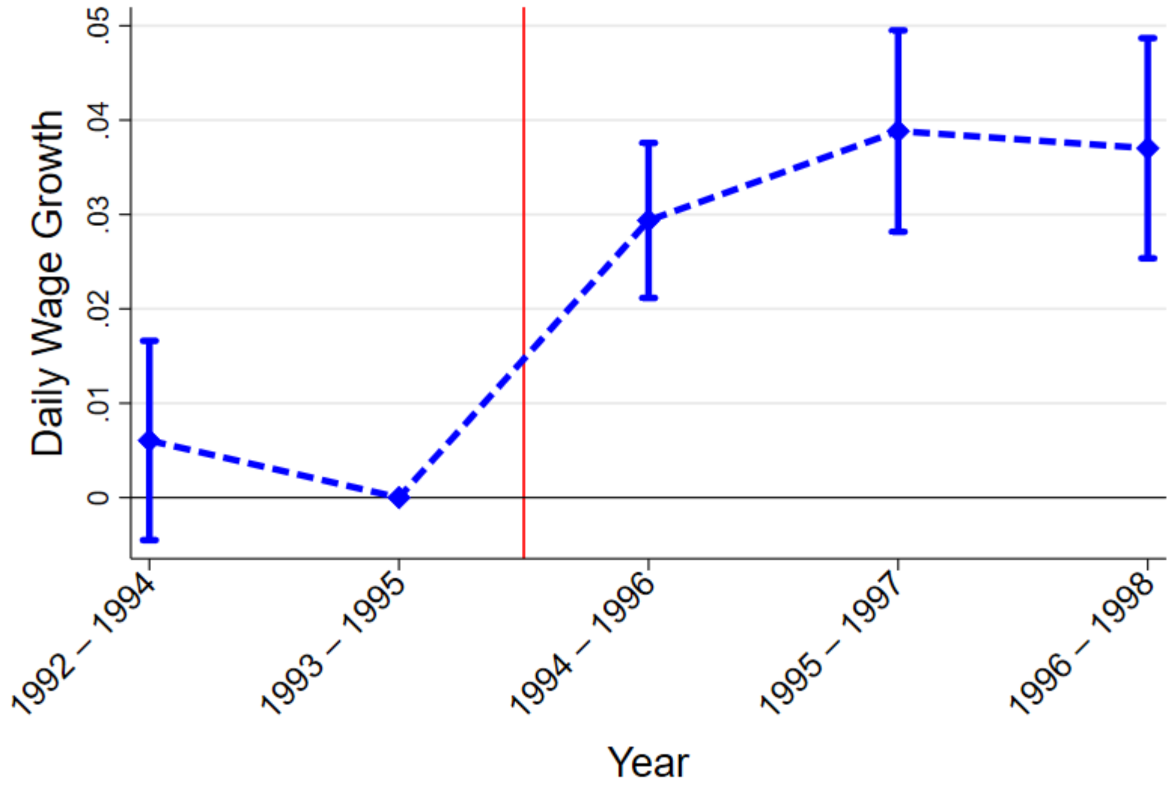
Appendix A Additional Figures and Tables

Figure A.1: Illustration of the Triple Differences Identification Strategy



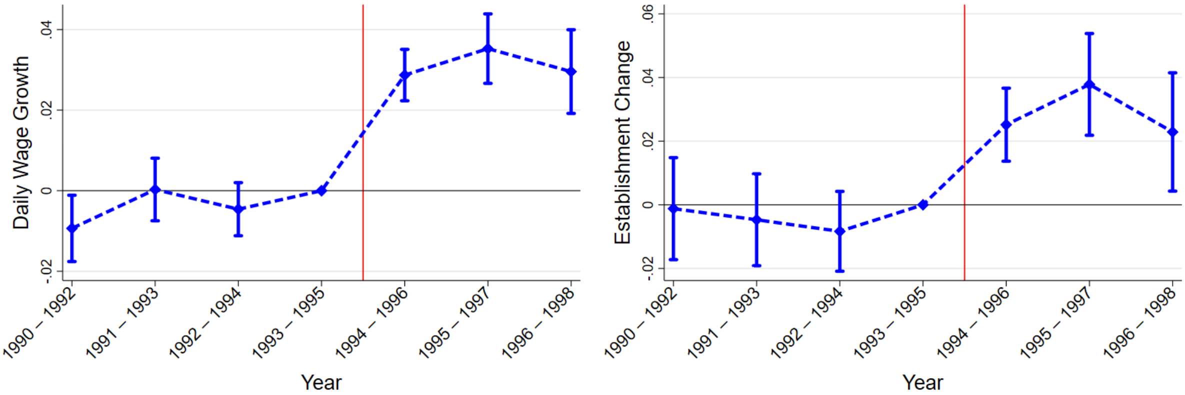
Notes: This figure illustrates the triple differences identification strategy from Equation 4. The green individuals in the top half of the figure represent the treated workers, while the orange individuals in the bottom half of the image represent the control group. The main construction sector and outside option industries share one common labor market. However, because the minimum wage was only implemented in the main construction sector, there is a dividing line between these two sectors. The area for the main construction sector is dot-filled gray because I concentrate on the spillover effects on the outside option industries in this paper rather than the within-sector effects. I expect this minimum wage to have spillover effects on treated workers in outside option industries. Non-outside option industries are outside this common labor market and serve as an additional control group.

Figure A.2: Difference-in-Differences: Wage Spillover Effects in East Germany



Notes: This figure illustrates the results of the difference-in-differences specification for treated versus control group workers, with the two-year change in log daily wages as the outcome variable (see Equation 2) for East Germany. The regression uses 73,116 worker-year observations with 19,612 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

Figure A.3: Difference-in-Differences: Excluding other Construction Industries from Outside Option Industries Classification

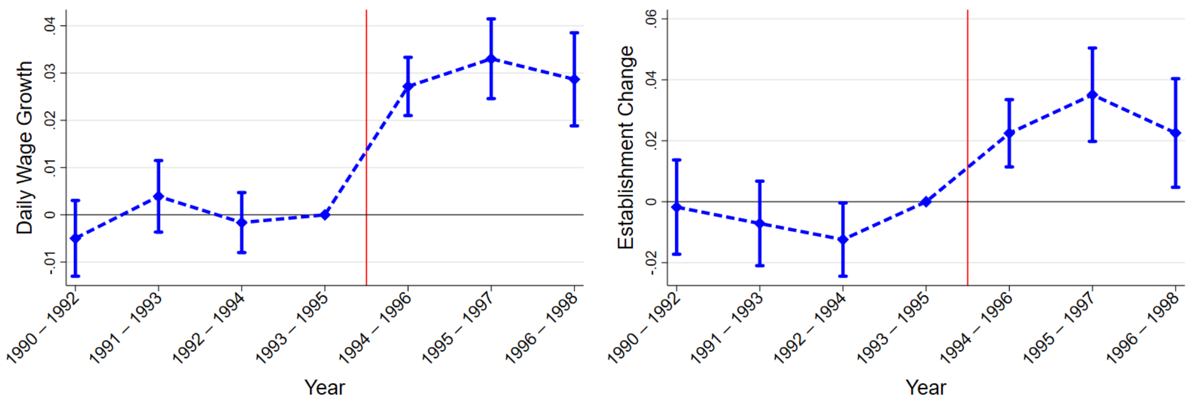


(a) Daily Wage Growth

(b) Detrended Establishment Change

Notes: This figure illustrates the results of the difference-in-differences specification for treated versus control group workers, with the two-year change in log daily wages (panel (a)) and change in establishments (panel (b)) as outcome variables (see Equation 2) and excluding other construction industries from the outside option industries classification (3-digit industries 451, 452, 454, 455). The regression uses 270,270 worker-year observations with 56,128 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. In panel (b), I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. **Source:** SIEED and BHP. Authors' calculations.

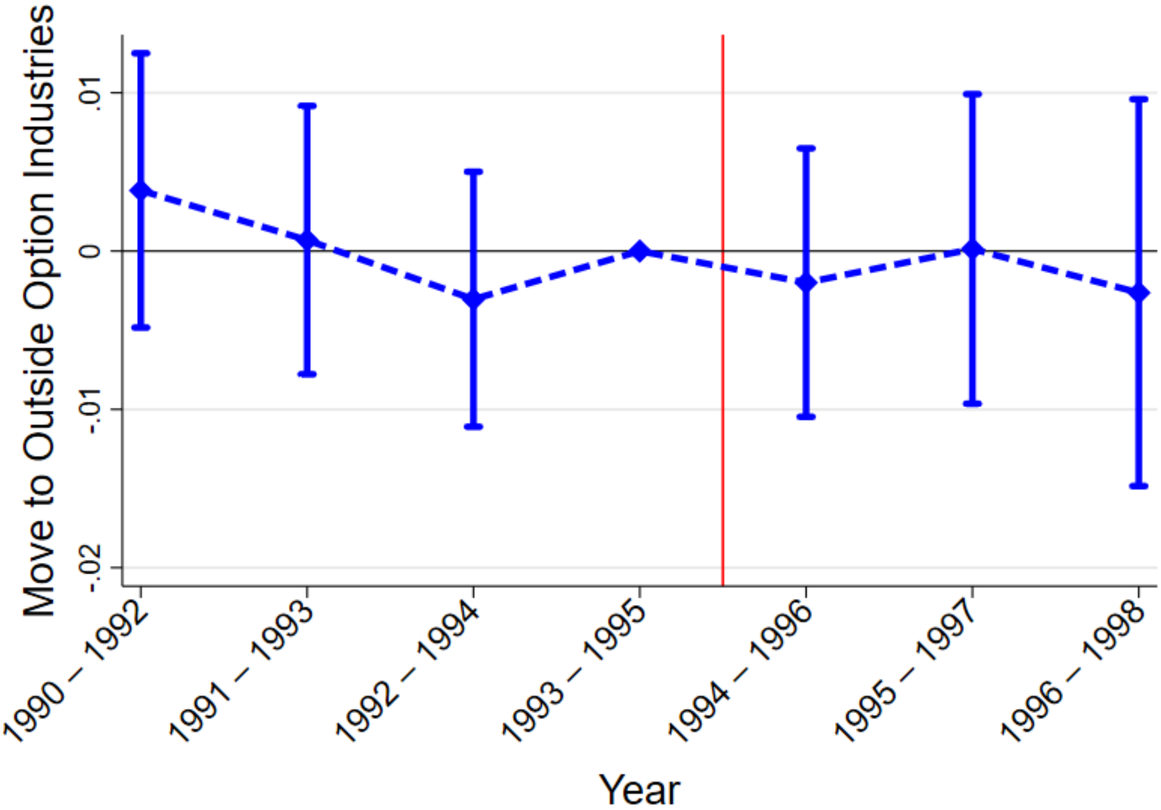
Figure A.4: Difference-in-Differences: Wage Spillover and Establishment Change Excluding Switches to Main Construction



(a) Daily Wage Growth (b) Detrended Establishment Change

Notes: This figure illustrates the results of the difference-in-differences specification for treated versus control group workers, with the two-year change in log daily wages (panel (a)) and change in establishments (panel (b)) as outcome variables (see Equation 2) and excluding switchers to the main construction sector from t to $t+2$. The regression uses 288,224 worker-year observations with 59,485 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. In panel (b), I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods.
Source: SIEED and BHP. Authors' calculations.

Figure A.5: Difference-in-Differences: Reallocation of Main Construction Sector Workers to Outside Option Industries



Notes: This figure illustrates the results of a difference-in-differences specification for treated versus control group workers, with the two-year change in the probability to move to outside option industries as the outcome variable. I use Equation 2 to estimate the DiD for the sub-sample of individuals who worked in the main construction sector in base period t. The regression uses 266,237 worker-year observations with 54,932 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. The coefficients are not detrended. **Source:** SIEED and BHP. Authors' calculations.

Table A.1: Classification of Sectoral Minimum Wages

Sector	**WZ73 (1975–2002)**	**WZ93 (1999–2003)**	**WZ03 (2003–2008)**	**WZ08 (from 2008)**	**First MW**
Main Construction	590/ 591/ 592/ 593/ 594/ 600/ 614	45.11.2/ 45.11.4/ 45.12.0/ 45.21.1-45.21.7/ 45.22.2/ 45.22.3/ 45.23.1/ 45.23.2-45.25.3/ 45.25.5/ 45.25.6/ 45.32.0/ 45.41.0/ 45.43.2/ 45.43.3/ 45.50.0	45.11.2/ 45.11.4/ 45.12.0/ 45.21.1-45.21.7/ 45.22.2/ 45.22.3/ 45.23.1-45.25.3/ 45.25.5/ 45.25.6/ 45.32.0/ 45.41.0/ 45.43.2/ 45.43.3/ 45.50.1/ 45.50.2	41.20.1-42.99.0/ 43.12.0/ 43.13.0/ 43.29.1/ 43.31.0/ 43.33.0/ 43.91.2-43.99.9	01/1997
Electrical Trade	611	45.31.0	45.31.0	43.21.0	06/1997
Roofing	601	45.22.1	45.22.1	43.91.1	10/1997
Painting & Varnishing	211/ 613	28.51.0/ 45.44.1	28.51.0/ 45.44.1	25.61.0/ 43.34.1	12/2003
Commercial Cleaning		74.70.1/ 74.70.3/ 74.70.4	74.70.1/ 74.70.3/ 74.70.4	81.21.0/ 81.22.9-81.29.9	04/2004
Waste Removal			37.10.1/ 37.10.2/ 37.20.1-37.20.5/ 90.02.1-90.02.5/ 90.03.0	38.11.0-39.00.0	01/2010
Nursing Care			85.31.5/ 85.31.7/ 85.32.6	87.10.0/ 88.10.1	08/2010
Security			74.60.2	80.10.0/ 80.20.0	06/2011
Temporary Work			74.50.2	78.20.0/ 78.30.0	01/2012
Scaffolding				43.99.1	08/2013
Stonemasonry				23.70.0	10/2013
Hairdressing				96.02.1	11/2013
Chimney Sweeping				81.22.1	04/2014
Slaughtering & Meat Processing				10.11.0-10.13.0	08/2014
Textile & Clothing				13.10.0-14.39.0	01/2015

Continued on next page

Table A.1 – continued from previous page

Sector	WZ73 (1975–2002)	WZ93 (1999–2003)	WZ03 (2003–2008)	WZ08 (from 2008)	First MW
Agriculture, Forestry & Gardening				01.11.0-02.40.0/ 03.12.0-03.22.0	01/2015

Table A.2: Descriptives for Minimum Wage Sectors ($t-5$ to $t-1$)

	Main Construction	Electrical Trade	Roofing	Painting & Varnishing	Commercial Cleaning	Waste Removal	Nursing Care	Security	Temporary Work	Scaffolding	Stonemasonry	Hairdressing	Chimney Sweeping	Slaughtering & Meat Processing
Panel A: West Germany														
Bite (for main sample restrictions)	5.82	9.38	5.73	6.89	26.81	2.18	15.24	13.86	28.55	39.35	10.27	46.22	5.27	11.78
Share in the economy	5.59	0.78	0.42	0.79	0.69	0.54	1.48	0.33	4.55	0.14	0.08	0.42	0.04	0.51
Share of full-time workers	93.35	79.78	89.31	76.00	19.05	82.79	38.98	55.93	72.38	74.62	62.15	39.97	50.00	58.78
Share of part-time workers	2.30	3.83	2.72	4.87	22.78	4.53	34.86	5.57	14.22	6.10	5.69	13.73	7.14	13.84
Share of women	9.11	16.29	8.07	21.45	69.05	16.81	80.57	19.03	43.62	8.87	19.85	91.87	36.67	56.33
Share of full-time women (full-time)	7.22	14.42	6.12	13.26	34.46	11.17	71.90	13.65	29.71	4.40	8.46	90.76	8.96	41.06
Share of full-time entrants	88.39	71.18	85.09	59.52	15.90	73.60	31.68	48.45	69.99	71.23	54.69	34.57	37.17	52.72
Share low-skill (full-time)	13.44	4.52	14.00	12.26	33.20	16.27	7.84	10.38	17.88	27.67	6.47	4.63	2.56	12.67
Share middle-skill (full-time)	79.33	93.16	83.32	84.51	57.90	77.10	81.50	84.53	70.82	63.98	88.56	93.51	96.38	80.39
Share high-skill (full-time)	5.88	1.77	2.44	2.55	4.62	5.43	10.00	3.80	10.23	4.12	2.89	1.11	0.64	5.94
Share non-German nationality (full-time)	15.32	8.70	10.46	10.90	32.12	5.40	3.92	8.89	16.67	34.76	26.07	9.33	0.43	10.04
Panel B: East Germany														
Bite (for main sample restrictions)	25.15	14.84	20.05	9.17	56.81	12.99	20.30	61.52	43.52	39.93	59.22	73.28	12.82	51.03
Share in the economy	10.04	1.48	0.39	1.06	1.47	1.32	2.57	0.63	3.82	0.11	0.09	0.67	0.03	0.64
Share of full-time workers	92.96	89.14	88.53	81.78	27.94	80.15	37.98	69.27	78.89	79.84	81.03	47.32	42.39	70.39
Share of part-time workers	1.24	2.53	1.33	1.83	33.94	3.76	48.51	4.15	7.97	5.94	6.32	31.31	16.85	11.74
Share of women	8.28	11.24	6.86	11.15	68.04	19.56	82.82	19.03	28.42	10.08	26.88	93.65	42.39	54.48
Share of full-time women (full-time)	7.68	9.72	6.45	8.76	59.93	14.86	77.84	15.48	17.90	8.09	22.44	93.13	3.85	54.27
Share of full-time entries	91.49	87.00	87.38	74.04	21.49	63.11	29.41	53.11	77.45	72.83	77.92	36.41	37.50	47.61
Share low-skill (full-time)	3.80	2.18	4.75	3.29	17.56	6.40	3.27	1.86	4.04	2.59	2.93	1.85	6.41	9.61
Share middle-skill (full-time)	89.66	92.12	92.90	94.17	75.34	85.34	82.36	91.39	92.14	93.20	88.78	97.16	88.46	75.41
Share high-skill (full-time)	5.70	4.56	1.11	2.15	3.82	7.78	13.52	6.52	3.61	3.56	5.37	0.87	0.00	4.52
Share non-German nationality (full-time)	3.32	1.28	1.24	1.15	9.89	0.51	1.48	0.47	0.78	0.65	4.88	0.38	0.00	12.72

Notes: This table shows descriptive statistics for the minimum wage sectors. The bite is calculated for the sample restrictions mentioned in Section 3.2. All other descriptives are calculated in each case in t-5 to t-1 before the introduction of the respective minimum wage using the full SIEED and BHP data. For example, the descriptives in column "Main Construction" are calculated from 1992 to 1996. All rows followed by the parentheses "(full-time)" are calculated by using the number of all full-time workers in the respective minimum wage sector as the denominator.
Source: SIEED and BHP. Authors' calculations.

Table A.3: List of Outside Option Industries (Main Construction Sector)

No.	Description
11	Growing of crops; market gardening; horticulture
12	Farming of animals
13	Growing of crops combined with farming of animals (mixed farming)
14	Agricultural and animal husbandry service activities, except veterinary activities
20	Forestry, logging and related service activities
102	Mining and agglomeration of lignite
103	Extraction and agglomeration of peat
111	Extraction of crude petroleum and natural gas
112	Service activities incidental to oil and gas extraction, excluding surveying
131	Mining of iron ores
141	Quarrying of stone
142	Quarrying of sand and clay
143	Mining of chemical and fertilizer minerals
144	Production of salt
145	Other mining and quarrying n.e.c.
201	Sawmilling and planing of wood; impregnation of wood
202	Manufacture of veneer sheets; manufacture of plywood, laminboard, particle board, fibre board and other panels and boards
203	Manufacture of builders' carpentry and joinery
204	Manufacture of wooden containers
261	Manufacture of glass and glass products
264	Manufacture of bricks, tiles and construction products, in baked clay
265	Manufacture of cement, lime and plaster
266	Manufacture of articles of concrete, plaster and cement
267	Cutting, shaping and finishing of stone
281	Manufacture of structural metal products
282	Manufacture of tanks, reservoirs and containers of metal; manufacture of central heating radiators and boilers
283	Manufacture of steam generators, except central heating hot water boilers
285	Treatment and coating of metals; general mechanical engineering
355	Manufacture of other transport equipment n.e.c.
361	Manufacture of furniture
364	Manufacture of sports goods
371	Recycling of metal waste and scrap
372	Recycling of non-metal waste and scrap
451	Site preparation
452	Building of complete constructions or parts thereof; civil engineering
454	Building completion
455	Renting of construction or demolition equipment with operator
701	Real estate activities with own property

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Table A.3 – continued from previous page

No.	Description
703	Real estate activities on a fee or contract basis
713	Renting of other machinery and equipment
742	Architectural and engineering activities and related technical consultancy
900	Sewage and refuse disposal, sanitation and similar activities

Table A.4: List of Non-Outside Option Industries (Main Construction Sector)

Industry	
No.	Description
15	Hunting, trapping and game propagation, including related service activities
233	Processing of nuclear fuel
242	Manufacture of pesticides and other agro-chemical products
403	Steam and hot water supply
523	Retail sale of pharmaceutical and medical goods, cosmetic and toilet articles
603	Transport via pipelines
621	Scheduled air transport
623	Space transport
642	Telecommunications
651	Monetary intermediation
724	Database activities
726	Other computer related activities
732	Research and experimental development on social sciences and humanities
801	Primary education
851	Human health activities
912	Activities of trade unions
924	News agency activities
930	Other service activities

Table A.5: Difference-in-Differences: Wage Spillover and Establishment Change Effects of the Main Construction Sector Minimum Wage

	Daily Wage Growth	**Establishment Change**
	(1)	(2)
Treated		
x 1990-92	-0.006	0.042***
	(0.004)	(0.008)
x 1991-93	0.002	0.022***
	(0.004)	(0.007)
x 1992-94	-0.004	0.004
	(0.003)	(0.006)
x 1994-96	0.029***	0.010*
	(0.003)	(0.006)
x 1995-97	0.036***	0.007
	(0.004)	(0.008)
x 1996-98	0.031***	-0.022**
	(0.005)	(0.009)
Linear trend	0.001	-0.015**
	(0.002)	(0.003)
Partial		
x 1990-92	0.000	0.015**
	(0.002)	(0.006)
x 1991-93	0.000	0.005
	(0.002)	(0.005)
x 1992-94	-0.003**	-0.006
	(0.001)	(0.004)
x 1994-96	0.010***	0.009**
	(0.001)	(0.004)
x 1995-97	0.010***	0.014***
	(0.002)	(0.005)

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Table A.5 – continued from previous page

	Daily Wage Growth	**Establishment Change**
	(1)	(2)
x 1996-98	0.011***	0.001
	(0.002)	(0.006)
Linear trend	0.000	-0.006
	(0.001)	(0.003)
No. of observations	292,562	292,562
No. of workers	60,310	60,310
Year fixed effects	yes	yes
3-digit Industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: This table shows the results of two difference-in-differences specifications with the two-year change in log daily wages and the two-year change in establishments as outcome variables (see Equation 2). Standard errors (in parentheses) are clustered at the worker level. In column (1), I present the DiD using the change in log daily wages as the outcome. In columns (2), I present the DiD using the change in establishments as the outcome. The rows *Linear trend (1990–95)* show the regression results, where I regress the 4 coefficients of the pre-period (including the reference period) on a time trend. The reference period is 1993–95. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table A.6: Tests of Strategic Complementarity Model Predictions in West Germany. Full Table

	Baseline	Tercile share	Bite
Treated x Post	0.033***	0.040***	0.033***
	(0.003)	(0.005)	(0.003)
Partial x Post	0.011***	0.016***	0.011***
	(0.001)	(0.002)	(0.001)
Treated x Middle x Post		-0.021***	
		(0.007)	
Treated x High x Post		-0.004	
		(0.007)	
Partial x Middle x Post		-0.011***	
		(0.003)	
Partial x High x Post		-0.005**	
		(0.003)	
Treated x Bite x Post			-0.001
			(0.003)
Partial x Bite x Post			-0.001
			(0.001)
No. of observations	292,562	288,869	286,871
No. of workers	60,310	59,574	59,171
Year fixed effects	yes	yes	yes
3-digit industry fixed effects	yes	yes	yes
Federal state fixed effects	yes	yes	yes
Region type fixed effects	yes	yes	yes
Worker fixed effects	yes	yes	yes
LMR fixed effects	no	yes	yes

Continued on next page

Table A.6 – continued from previous page

	Baseline	Tercile share	Bite

Notes: Standard errors in parentheses. The table shows specifications using Equation 3 with the 2-year change in log (daily) wages as the outcome for West Germany. The first column shows the baseline estimation, also illustrated in Table 3. In the second column, I interact the baseline DiD interaction additionally with the terciles of the share of the main construction sector within a LMR. I also include the indicator variable for tercile as an additional control. "Middle" indicates workers who work in a LMR in the middle tercile of the employment weighted distribution of shares of the main construction sector and "High" indicates workers in the highest tercile of this distribution. In the third column, I interact the baseline DiD interaction additionally with the bite of the main construction sector minimum wage in each LMR. I calculate the bite as the share of employees who earned below the first minimum wage threshold in the pre-period within the main construction sector. I standardize the bite measure to have mean zero and standard deviation 1. All specifications include the baseline fixed effects: year, 3-digit industry, federal state, region type, and worker. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

Table A.7: Tests of Strategic Complementarity Model Predictions in East Germany. Full Table

	Baseline	Tercile share	Bite
Treated x Post	0.032***	0.047***	0.034***
	(0.004)	(0.017)	(0.004)
Partial x Post	0.007**	0.007	0.009**
	(0.004)	(0.015)	(0.004)
Treated x Middle x Post		-0.020	
		(0.019)	
Treated x High x Post		-0.016	
		(0.018)	
Partial x Middle x Post		0.003	
		(0.017)	
Partial x High x Post		0.000	
		(0.016)	
Treated x Bite x Post			0.008*
			(0.004)
Partial x Bite x Post			-0.004
			(0.004)
No. of observations	73,116	73,116	73,116
No. of workers	19,612	19,612	19,612
Year fixed effects	yes	yes	yes
3-digit industry fixed effects	yes	yes	yes
Federal state fixed effects	yes	yes	yes
Region type fixed effects	yes	yes	yes
Worker fixed effects	yes	yes	yes
LMR fixed effects	no	yes	yes

Continued on next page

Table A.7 – continued from previous page

	Baseline	Tercile share	Bite

Notes: Standard errors in parentheses. The table shows specifications using Equation 3 with the 2-year change in log (daily) wages as the outcome for East Germany. The first column shows the baseline estimation, also illustrated in Table 3. In the second column, I interact the baseline DiD interaction additionally with the terciles of the share of the main construction sector within a LMR. I also include the indicator variable for tercile as an additional control. "Middle" indicates workers who work in a LMR in the middle tercile of the employment weighted distribution of shares of the main construction sector and "High" indicates workers in the highest tercile of this distribution. In the third column, I interact the baseline DiD interaction additionally with the bite of the main construction sector minimum wage in each LMR. I calculate the bite as the share of employees who earned below the first minimum wage threshold in the pre-period within the main construction sector. I standardize the bite measure to have mean zero and standard deviation 1. All specifications include the baseline fixed effects: year, 3-digit industry, federal state, region type, and worker. Significance: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

Appendix B Establishment Level Analysis

B.1 Establishment Exposure

To estimate establishment level responses to the main construction sector minimum wage, I keep only panel establishments that were sampled in a first step for the data (see Section 3.1) and collapse the worker level data to the establishment level.

In the establishment level approach, I exploit the continuous variation in the exposure to the main construction sector minimum wage across establishments. This approach is based on a large literature exploiting regional variation in the bite of federal minimum wages (e.g. Card, 1992; Bailey et al., 2021; Dustmann et al., 2022). Derenoncourt and Weil (2024) and Bossler and Gerner (2020) recently employed this method to examine exposure to minimum wages across employer-by-occupation-by-commuting-zone cells and establishments, respectively. Formally, I define the exposure D_j of an establishment j to the main construction minimum wage as

$$D_j = \frac{\sum_{i \in j} \sum_{t \in [1990,1995]} \mathbb{1}(h_{i,t} < 8.69 + 40\%)}{N_{j,t \in [1990,1995]}}, \tag{6}$$

where $N_{j,t \in [1990,1995]}$ is the number of workers in an establishment for the time period 1990–95. Thus, I define exposure of an establishment to the main construction sector minimum wage as the fraction of workers paid a nominal hourly wage below the threshold for partially treated workers in the pre-introduction period of 1990-95.

Figure B.1 shows the distribution of the exposure measure across establishments. Many establishments pay all of their workers an hourly wage below the cutoff. These establishments are characterized by a very small number of workers (1–4 workers), which naturally makes it more likely to have an exposure value of 1. Apart from this, the figure shows a continuous and relatively uniform distribution across exposure bins.

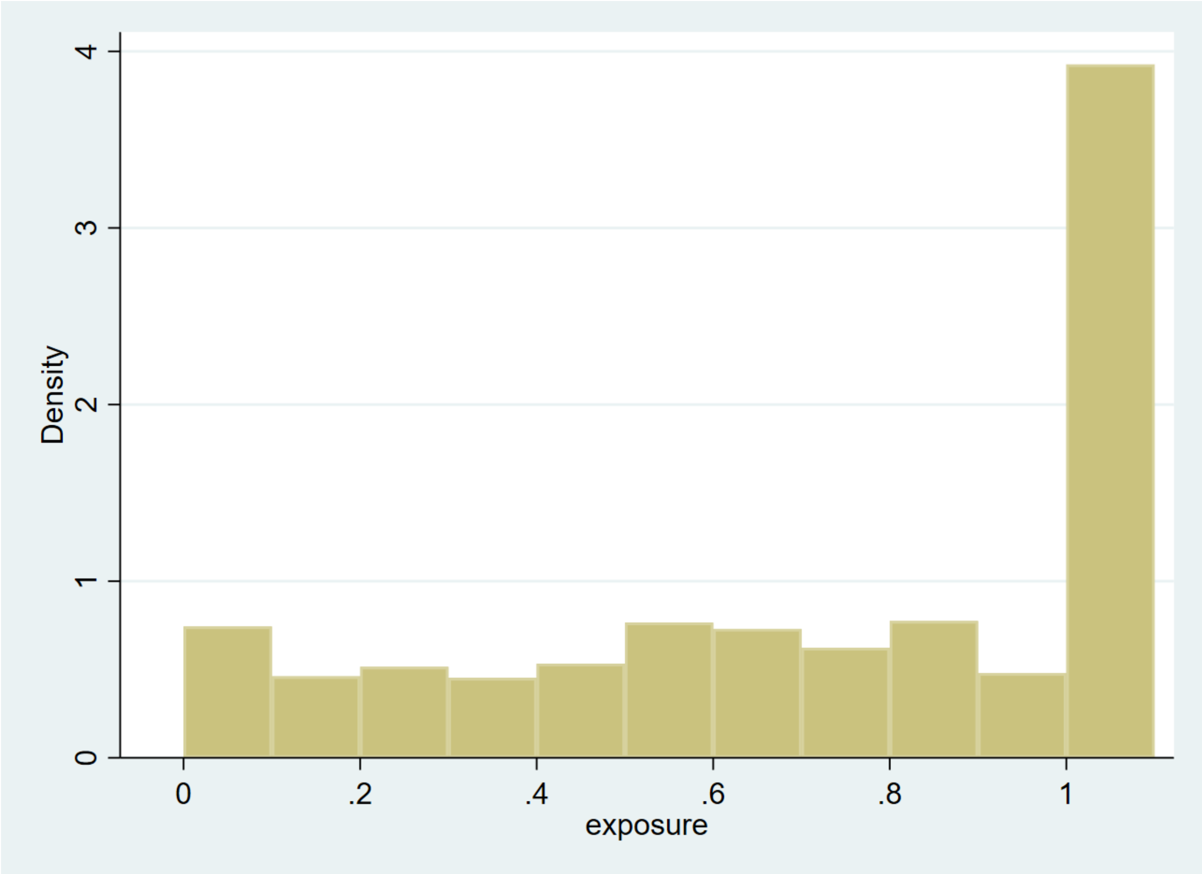
B.2 Empirical Strategy

To analyze the horizontal spillover effects from the main construction sector minimum wage on establishments, I exploit the continuous variation in the exposure D_j of an establishment j in the following event-study DiD specification for the subset of outside option industries:

$$y_{j,t} = \alpha_j + \zeta_t + \sum_{t=1990, t \neq 1995}^{1999} \gamma_t D_j \times Year_t + \epsilon_{j,t}. \tag{7}$$

$y_{j,t}$ denotes the log (daily) average wage, α_j are establishment fixed effects, and ζ_t are year fixed effects. At the establishment level, there is not a comparable problem of mean reversion as at the worker level, which allows to focus on the growth of outcomes rather than changes in growth. The coefficients γ_t track how establishments with higher exposure to the main construction sector minimum wage responded to it relative to establishments with lower exposure and relative to the base year 1995. For the years $t > 1995$, the coefficient estimates for γ_t provide the causal spillover effect of the main construction minimum wage if the parallel trends assumption holds. Specifically, the assumption underlying the DiD specification in Equation 7 is that more exposed establishments would have evolved similarly in terms of potential outcomes compared to less exposed establishments in the absence of the main construction minimum wage. In Section B.3, I provide suggestive evidence for this parallel trends assumption by visualizing the coefficient estimates for γ_t for the years prior to the minimum wage announcement

Figure B.1: Density of the Continuous Establishment Exposure Measure



Notes: For this figure, I keep only one observation per establishment in the period 1990–95. **Source:** SIEED and BHP 1990–95. Authors' calculations.

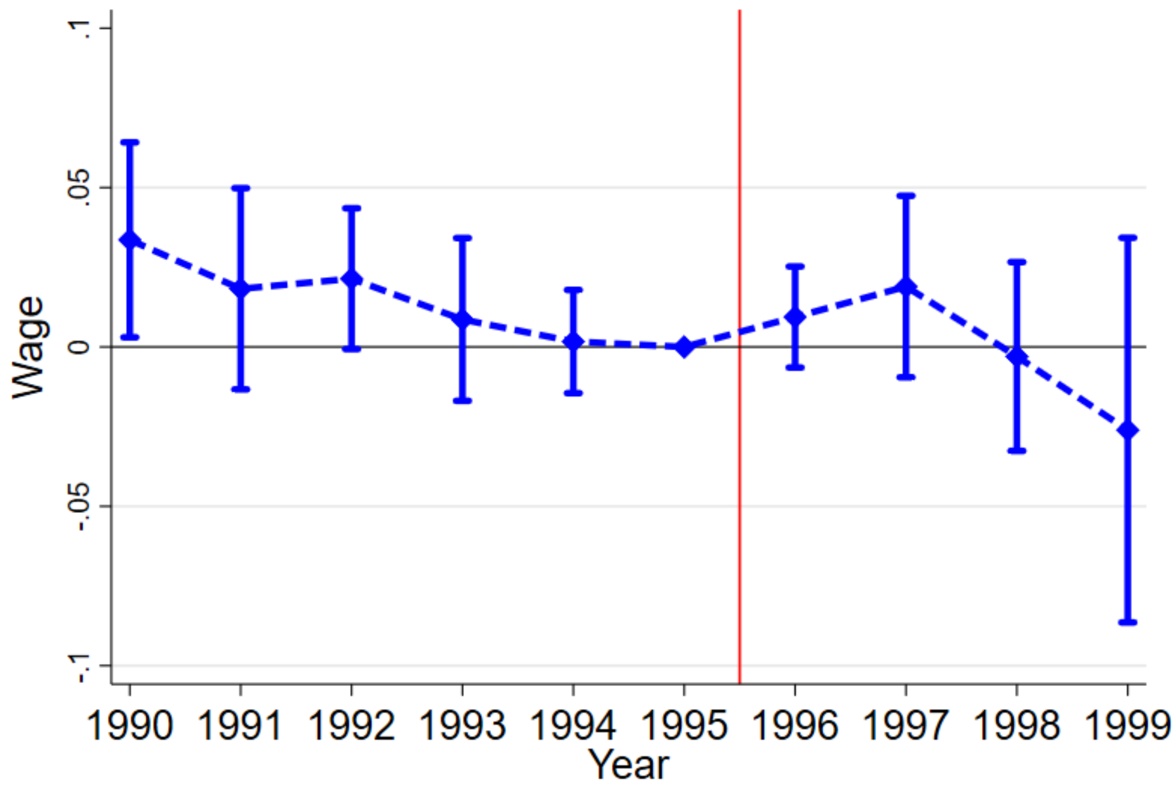
$t < 1995$. Coefficient estimates for $t < 1995$ that are statistically and/or economically insignificant indicate a plausible parallel trends assumption. I cluster standard errors at the establishment level. I weight the regressions by using the average number of full-time employees within each establishment in the 1990–95 pre-period.

B.3 Results

To shed light on demand-side responses and to compare the results with the existing empirical evidence on cross-employer spillover effects (Staiger et al., 2010; Derenoncourt and Weil, 2024; Bassier, 2022), I analyze the horizontal spillover effects from the main construction sector minimum wage from the perspective of establishments.

Figure B.2 plots the coefficient estimates for γ_t for the DiD specification from Equation 7. The outcome variable is the log (daily) average wage of an establishment. I find no statistically significant effect on average wages on more exposed establishments using the DiD specification. Consistent with this establishment level results, in Section 5.3, I show that the wage spillover results were mainly driven by the switching behavior of treated workers, and that those treated workers who stayed did not experience a significant change in wages. Moreover, treated workers reallocated from low-wage to high-wage employers.

Figure B.2: Establishment Level: Wage Spillovers from the Main Construction Sector Minimum Wage



Notes: The outcome variable is the log (daily) average wage. In this figure, I illustrate the results of Equation 7. The estimation is weighted by the average number of full-time employees within establishments in the 1990–95 pre-period. The regression uses 17,737 establishment-year observations with 2,234 establishments. I use 95% confidence intervals. **Source:** SIEED and BHP 1990–99. Author's calculations.

Appendix C Wage and Employment Effects within the Main Construction Sector

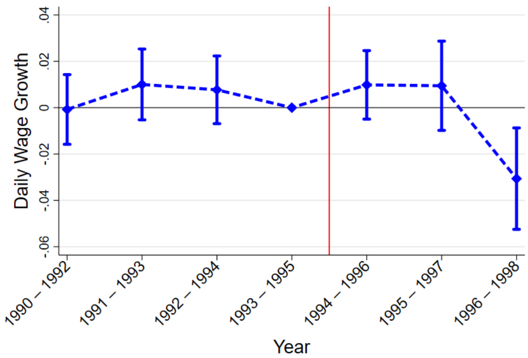
This appendix examines the wage and employment effects of the minimum wage in the main construction sector. I contrast the effects for the West German sample with the effects for the East German sample. I use three different estimation approaches. First, I use the same specification as the difference-in-differences specification for treated versus control group workers in Equation 2 for the sample of main construction workers. I find no or only negative wage growth effects and positive establishment change effects in the announcement year and negative effects in the introduction year in West Germany. In contrast, I find positive wage growth effects and no establishment change effects in East Germany. Second, I use a regional bite measure of the minimum wage, following an established literature on these measures, to estimate minimum wage effects (e.g. Card, 1992; Bailey et al., 2021). With this measure I can replicate the main findings of Vom Berge and Frings (2020), namely no wage and employment effects of the main construction minimum wage for West Germany, but positive wage effects in East Germany, however with no employment effects. Third, extending Vom Berge and Frings (2020), I use an establishment level bite measure, similar to the approach used by Bossler and Gerner (2020) for the federal minimum wage. The advantage of the establishment level approach is that it allows for the detection of wage and employment effects even if they occur within regions, e.g. because a highly exposed establishment is located in a low exposed region (Bossler and Gerner, 2020). For West Germany, I find an increase in average wages for establishments that are more exposed to the minimum wage, but with no effect on employment. Together with the worker-level results, this implies a change in composition for more exposed establishments in West Germany. For East Germany, I also find an increase in average wages for more exposed establishments and a negative effect on employment for these employers.

C.1 Worker Level

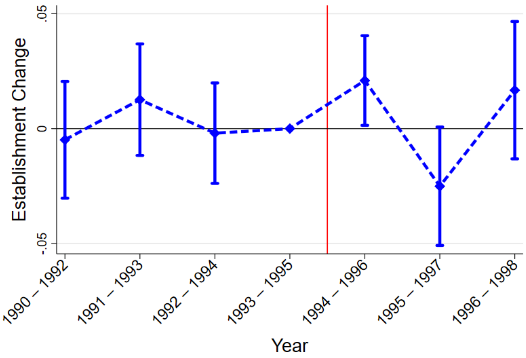
To allow a comparison of the within-sector effects of the main construction minimum wage with the spillover effects to the outside option industries, I estimate the main specification Equation 2 using the sub-sample of workers in the main construction sector instead of workers in the outside option industries. Because I detect statistically significant group-specific linear trends, I present detrended coefficients and confidence intervals for West Germany. Due to the lack of pre-periods for East Germany, I cannot test for the presence of linear trends there. Figure C.1 illustrates the results. I find no wage growth effects and positive establishment change effects in the announcement year and negative establishment change effects in the introduction year for West Germany. The absence of positive wage effects at the worker level and positive average wage effects at the establishment level (in Figure C.3) suggests changes in the composition of workers within the main construction sector in West Germany. Consistent with this interpretation, establishment changes increase at the worker level in the year of the announcement. Note, however, that these workers did not move to outside option industries (see Figure A.5). Since in this paper I am interested in the horizontal spillover effects of the main construction minimum wage on outside option industries, a detailed analysis of all patterns of this minimum wage within the main construction sector is beyond the scope of this paper.

In contrast, I find positive wage growth effects and no effects on establishment changes for workers in the main construction sector in East Germany.

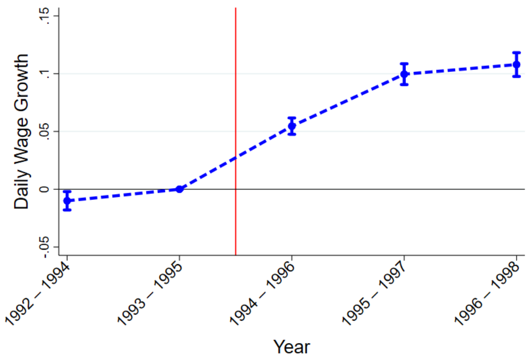
Figure C.1: Wage and Establishment Change Effects within the Main Construction Sector



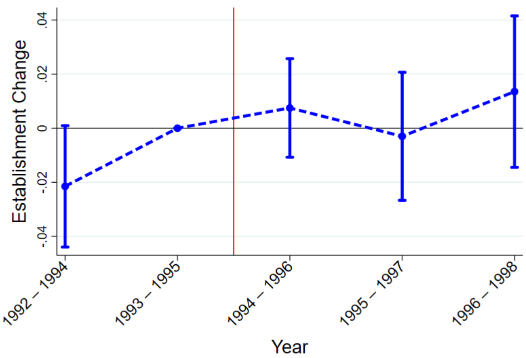
(a) West Germany: Detrended Daily Wage Growth



(b) West Germany: Detrended Establishment Change



(c) East Germany: Daily Wage Growth



(d) East Germany: Establishment Change

Notes: This figure illustrates the results of the difference-in-differences specification for treated versus control group workers, with the two-year change in log daily wages (panel (a) and (c)) and change in establishments (panel (b) and (d)) as outcome variables (see Equation 2) for the sample of workers in the main construction sector. Panels (a) and (b) use the sample of workers in West Germany with 266,237 worker-year observations and 54,932 workers. Panels (c) and (d) use the sample of workers in East Germany with 130,029 worker-year observations and 34,003 workers. I use 95% confidence intervals. Control variables include: year fixed effects, 3-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. In panels (a) and (b), I present detrended coefficients and confidence intervals by estimating a linear time trend for the 1990–95 pre-announcement period and extrapolating it to the post-announcement periods. **Source:** SIEED and BHP. Authors' calculations.

C.2 Regional Bite Measure

To calculate the main construction sectors' minimum wage bite for each of the 106 labor market regions in my West German sample, I calculate the share of workers earning a wage below 8.69 Euro in each labor market region[42] for the years 1992–96 within the main construction sector. The median (mean) bite across labor market regions in this analysis sample is 5.1% (7.3%) with a standard deviation of 5.9%. For East Germany, I proceed similarly and calculate the main construction sectors' minimum wage bite for each of the 38 labor market regions in my East German sample. The median (mean) bite across labor market regions in the East German analysis sample is 20.6% (26.4%) with a standard deviation of 13.4%.

[42]To define labor market regions, I follow Kosfeld and Werner (2012).

Formally, I estimate the following DiD specification for the sub-sample of the main construction sector around the time of the public discussion and announcement of the main construction sector minimum wage:

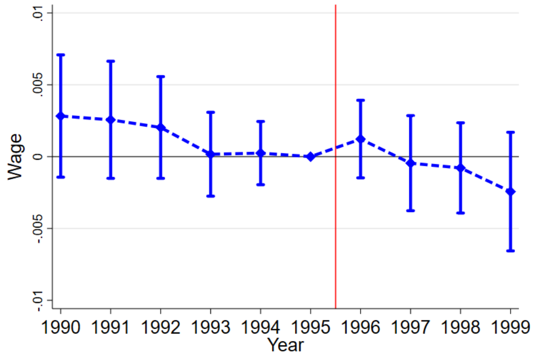
$$y_{i,t} = \alpha_r + \zeta_t + \upsilon_g + \sum_{t=1990, t\neq 1995}^{1999} \gamma_t D_r \times Year_t + \delta X_{i,t} + \epsilon_{i,t}. \quad (8)$$

$y_{i,t}$ denotes either the log (daily) wage or the employment probability within the main construction sector of individual i in year t. α_r are labor market region fixed effects, ζ_t are year fixed effects, and υ_g are 3-digit industry fixed effects. The parameter of interest in this equation is γ_t, which measures the DiD of labor market regions with a high bite vs. lower bite (D_r) over time. Finally, I also include federal state and region type (on the county level) fixed effects in $X_{i,t}$. The standard errors are clustered at the labor market region level. Note that the analysis period for East Germany does not begin until 1992.

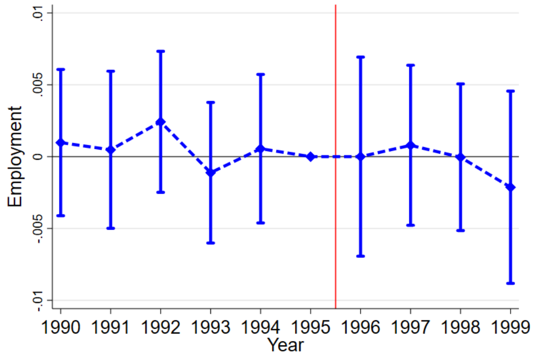
Figure C.2 illustrates the results. Using the regional bite measure, I find no wage or employment effects of the minimum wage within the main construction sector in West Germany. This confirms the results in Vom Berge and Frings (2020) for West Germany.[43] However, as in Vom Berge and Frings (2020), I find positive wage effects for East Germany. The results show no employment effects at the regional level for East Germany.

[43] In unreported results, I use worker fixed effects and find no effects in this specification either.

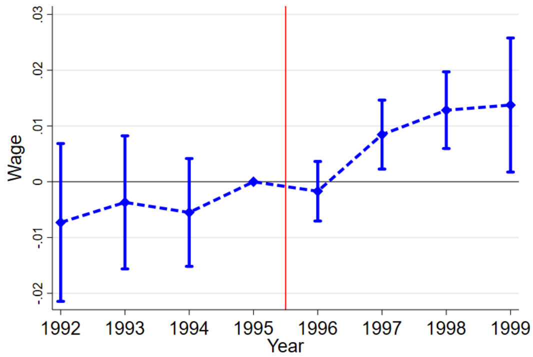
Figure C.2: Regional Bite of the Minimum Wage: Wage and Employment Effects within the Main Construction Sector



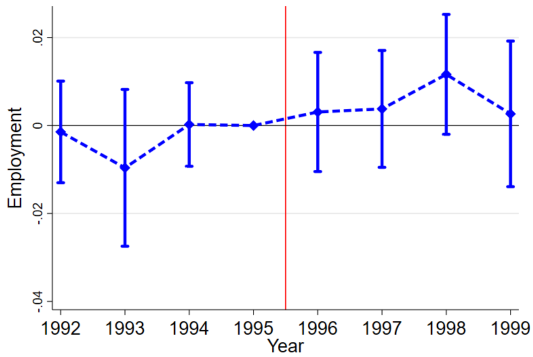
(a) West Germany: Daily Wage



(b) West Germany: Employment



(c) East Germany: Daily Wage



(d) East Germany: Employment

Notes: In this figure, I illustrate the results of Equation 8 for the sample of workers in the main construction sector. Panels (a) and (b) use the sample of workers in West Germany with 820,161 worker-year observations and 106 labor market regions. Panels (c) and (d) use the sample of workers in East Germany with 396,852 worker-year observations and 38 labor market regions. I use 95% confidence intervals. The standard errors are clustered at the labor market region level. **Source:** SIEED and BHP 1990–99. Author's calculations.

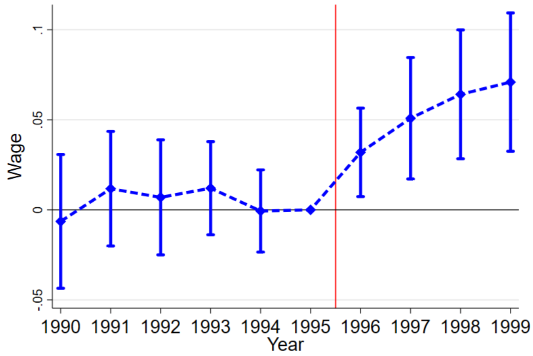
C.3 Establishment Bite Measure

In this section, I present establishment level results on wage and employment effects within the main construction sector. The definition of the key independent variable is the same as in Section B.1 and I use the empirical strategy of Section B.2 separately on the sample of establishments in the main construction sector in West Germany and East Germany.

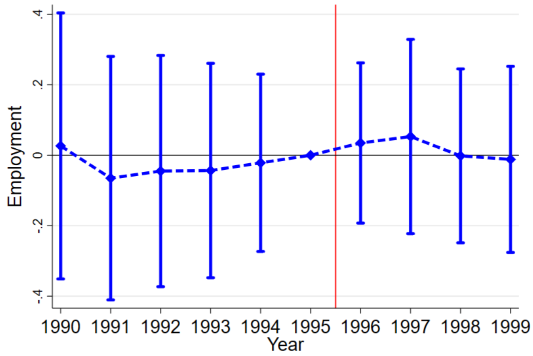
Figure C.3 illustrates the results. After accounting for the linear pre-trends in all panels of Figure C.3, I find an increase in wages but no effect on employment for establishments within the main construction sector that were more exposed to the minimum wage in West Germany. As noted in Section C.1, the increase in average wages in more exposed establishments combined with no effect on wage growth at the worker level in West Germany suggests a compositional change within these establishments rather than an increase in wages for low-wage workers.

In contrast, average wages also increased in more exposed establishments within the main construction sector in East Germany, but with negative employment effects within these establishments. Combined with the results in C.1 at the worker level, the results here suggest an increase in wages also for low-wage workers, driving the increase in average wages for exposed establishments in East Germany.

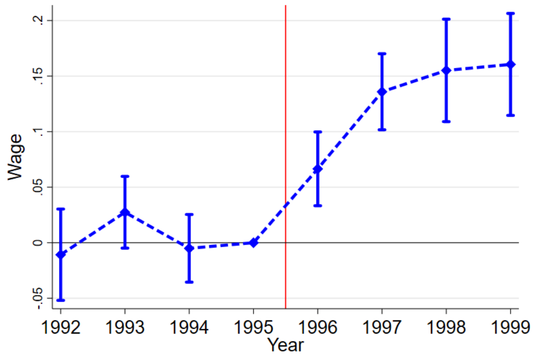
Figure C.3: Establishment Level: Wage and Employment Effects within the Main Construction Sector



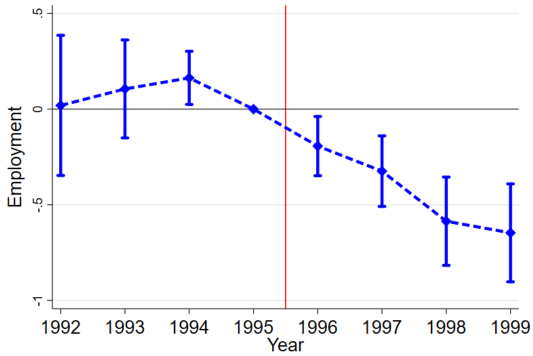
(a) West Germany: Detrended Daily Wage



(b) West Germany: Detrended Employment



(c) East Germany: Detrended Daily Wage



(d) East Germany: Detrended Employment

Notes: In this figure, I illustrate the results of Equation 7 for the sample of establishments in the main construction sector. The outcome variable in panels (a) and (c) is the log average daily wage and in panels (b) and (d) it is the log number of full-time employed workers (according to sample restrictions). The estimation is weighted by the average number of full-time employees within establishments in the 1990–95 (1992–95 in East Germany) pre-period. Panels (a) and (b) use 8,814 establishment-year observations with 1,078 establishments. Panels (c) and (d) use 2,457 establishment-year observations with 393 establishments. I use 95% confidence intervals. In all panels, I use the estimated linear time trend from the pre-period and extrapolated to the post-period to present the detrended coefficients and confidence intervals. **Source:** SIEED and BHP 1990–99. Author's calculations.

Appendix D Strategic Complementarity

D.1 Theoretical Model

Suppose that workers are uniformly distributed along a straight line. Two sectors, A and B, are located at distance d_r from each other at the straight line. The distance d_r between the two sectors can vary by local labor market region (LMR) r. I assume that each LMR is a closed labor market. Workers have to pay transportation costs τ for each distance unit traveled. An individual located at x_r^* distance units from sector A is indifferent between working for sector A or sector B if:

$$w_r^A - \tau x_r^* = w_r^B - \tau(d_r - x_r^*), \tag{9}$$

where sector A pays wage w_r^A in LMR r and sector B pays w_r^B. Solving for x_r^* gives:

$$x_r^* = \frac{w_r^A - w_r^B + d_r\tau}{2\tau}. \tag{10}$$

This point of indifference, x_r^*, is sector A's labor supply L_r^A.

Each firm in the respective sectors maximizes profits given β, the marginal benefit of employing a worker. Substituting labor supply into the profit maximization problem and then solving for the optimal wage using the first-order condition provides the wage-setting equation in this model:

$$w_r^A = \frac{\beta + w_r^B - d_r\tau}{2}. \tag{11}$$

Wages increase with β and the wage of competitor B. Sector A can set its wages more independently from sector B's wages (and vice versa) whenever the distance between these two sectors is larger. The optimal labor demand given labor supply is:

$$L_r^A = \frac{\beta + d_r\tau - w_r^B}{4\tau}. \tag{12}$$

Labor in sector A increases with β and decreases with the wage in sector B. Furthermore, labor supply to sector A is larger whenever the distance to the competitor is larger.

D.2 Additional Variables in the SIEED

For the empirical tests, I calculate the share of the main construction sector in a labor market region. I proceed in four steps and use the delineation of labor market regions from Kosfeld and Werner (2012). First, I use the raw data and keep only panel establishments. Second, for each labor market region, I calculate the relative share of full-time workers in the main construction sector using only the pre-introduction years 1992–96. Third, I split the distribution of shares of main construction sector full-time workers across labor market regions into terciles, weighted by the number of full-time workers in each labor market region. Fourth, I merge this information to my sample. LMRs in the lowest tercile have shares of the main construction sector that range from 0% to 4%, LMRs in the middle tercile have shares of the main construction sector that range from 4.1% to 7.2%, and LMRs in the highest tercile have shares of the main construction sector that range from 7.2% to 36.9%.

Appendix E Biased Beliefs about Outside Options

E.1 Theoretical Model

In this section, I sketch the theoretical model in Jäger et al. (2024).

In the model, first N homogenous firms enter the labor market. Then, L workers are randomly assigned to firms and supply labor inelastically. Workers learn their wages and potentially update their beliefs about the external wage distribution. Assume the existence of two types of workers who differ in their cost to gather complete information about the labor market. A share α of workers are experts who face no information costs $c_E = 0$ and are always perfectly informed about their outside options in the labor market. The remaining share $1 - \alpha$ are amateur workers who face information costs $c_A > 0$ and can therefore form biased beliefs about their outside options. Amateur's job search decision depends on their beliefs about the benefits of job search

$$\widetilde{w}^{max}(w_j, w_{j-1}) - w_j > c_A, \tag{13}$$

where w_j is the wage of a worker in her current firm j. $\widetilde{w}^{max}(w_j, w_{j-1})$ is the belief about the highest wage. Thus, workers search for new jobs if they believe that the wage they could potentially earn is higher than their current wage plus search costs. The belief about the highest potential wage is a weighted average of the actual highest wage and worker's current wage:

$$\widetilde{w}^{max} = \gamma w_j + (1 - \gamma) w^{max}. \tag{14}$$

The variable $\gamma \in [0, 1]$ captures the degree of anchoring on the current wage. If, e.g., $\gamma = 1$ then workers fully anchor their belief about potential outside options on their current wage. With $\gamma = 0$, workers have accurate beliefs. Empirically, Jäger et al. (2024) show that especially low-wage workers anchor their beliefs about outside options on their current wage and therefore underestimate wages elsewhere.

In the theoretical model, firms maximize their profits given the labor costs per worker. The competitive wage is w^* and equals the marginal product of labor. Jäger et al. (2024) also model how a segmented labor market of firms paying the competitive wage (high-wage firms) and firms paying a marked down wage (low-wage firms) can emerge. For such a segmented labor market to emerge, the only profitable departure from the competitive wage w^* is to pay a wage below w^*, but still large enough to retain a firm's stock of amateur workers. Any downward deviation from the competitive wage will result in an immediate loss of a firm's stock of expert workers.

The reservation wage of amateur workers to not become informed is given by Equation 13. The most profitable deviation is to exactly pay the reservation wage. Considering the formation of biased beliefs in Equation 14 and using it in Equation 13 gives:

$$w' = w^* - \frac{c_A}{1 - \gamma}. \tag{15}$$

w' is the most profitable deviation and represents a markdown of the competitive wage w^*. The markdown from the competitive wage is higher with higher information costs c_A and higher anchoring γ. Deviant firms only retain their amateur workforce and therefore employment in these firms is

$$l(w') = (1 - \alpha) \frac{L}{N}. \tag{16}$$

The deviant wage w' and employment $l(w')$ describe the behavior of low-wage firms in the labor market. For completeness, high-wage firms pay the competitive wage and employ all expert workers in the labor market (plus a share of amateur workers who initially sorted into those firms).

E.2 Additional Variables

Abowd et al. (1999) (hereafter AKM) introduced an estimation strategy to isolate worker-specific and establishment-specific wage premia by using additive fixed effects for workers and establishments. Card et al. (2013) use the AKM estimation strategy to study the role of establishment-specific wage premia in generating recent increases in wage inequality in West Germany. The establishment-specific wage premia can be interpreted as a proportional pay premium or discount that is paid by an establishment to all employees, e.g., due to rent-sharing, efficiency wage premium, or strategic wage posting behavior (Card et al., 2013). The estimation strategy of AKM requires a connected set of establishments linked by worker mobility to identify the fixed effects. I use the AKM establishment fixed effects provided by Bellmann et al. (2020) and estimated for the universe of workers and establishments in the German social security data. These estimated AKM establishment fixed effects are available for the five sub-periods 1985–92, 1993–99, 1998–2004, 2003–10, and 2010–17.